Archives of Neurology and Psychiatry

VOLUME 19 JUNE, 1928 NUMBER 6

EVOLUTION AND THE DARWINIAN THEORY OF HUMAN DESCENT

VIEWED FROM THE STANDPOINT OF A MULTIPLE PRIMATE ANCESTRY *

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The Philadelphia Neurological Society was organized late in 1884. In the early years of the society, especially in the lustrum from 1885 to 1890, much attention was given in its proceedings to the morphology, anatomy and physiology of the brain. The reason for this is to be sought in a knowledge of the character and scientific tendencies of some of the early members, among whom I may refer to, Andrew J. Parker, Francis X. Dercum, Harrison Allen, James Hendrie Lloyd, William Osler and perhaps myself.

Parker and Dercum at times pursued investigations in the laboratory of that great biologist, Joseph Leidy. In Leidy's laboratory, with Parker, I joined in some researches on the circulation of the brain and on the surface morphology of Chinese, negro and aberrant Caucasian brains.

Leidy was an early disciple of Darwin, with whom he not infrequently corresponded. Influenced by the example of Leidy and our own personal inclinations toward new scientific truths, it happened, therefore, that some of us soon became ardent Darwinians. Some of the work contributed at the meetings of our Neurological Society, in addition to that from Leidy's laboratory, was done at the Academy of Natural Sciences and some at the Philadelphia Hospital.

Osler came to Philadelphia to become professor of clinical medicine in the University of Pennsylvania in 1884, and soon became a member and a constant attendant at the meetings of our society. My attention was first attracted to his work by a paper on "The Brains of Criminals." [1] Like Osler, I had studied the papers of Benedickt of Vienna on this subject, and while not fully subscribing to the views of Benedickt, I had found many confirmations of his ideas in the numerous brains of the low types which I examined.

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* Read at a Meeting of the Philadelphia Neurological Society, Nov. 18, 1928. A number of other papers in the symposium on Evolution, read at the same meeting, appear in the Society Proceedings in this issue.

1. Osler, William: The Brains of Criminals, Canad. M. & Surg. J. February, 1872.

Probably no one has done so much for American archeology as Dr. Daniel Garrison Brinton, of Philadelphia. Not a little of his work was contributed to the proceedings of the Academy of Natural Sciences of Philadelphia.

In 1895, a Section of Anthropology was started at the Academy of Natural Sciences with Dr. Harrison Allen as director. On this section Dr. Dercum and I were in frequent attendance. This section continued to function during 1895, 1896 and 1897 and was discontinued in 1898. I recall that at one of the meetings Brinton exhibited and described a number of skulls from South America, chiefly from Peru.

Brinton, in his work on "The American Race" [2] and in his contributions on American archeology, takes the ground which supports the main contention of this paper, that of a separate primate ancestry for the Red Indian. He holds that the American Indian is a particular and separate race, which was on the American continent before the glacial period many thousand years ago. He opposes the view that the American Indian comes from Asia, a view that sprang from the idea that the human race had its origin in Asia from a single family tree.

Brinton contends that at an early period there was a land communication between the northernmost portion of North America and the northern islands of the Atlantic—Greenland, Iceland, Shetland, Ireland, Wales and England. He gives many facts, linguistic and paleontologic, which point to the idea that the race which inhabited the northernmost part of North America was the same as that of which the remains are found in England, Shetland, Ireland, Scotland and Wales.

Some differences of opinion exist as to whether the Eskimos or Innuits belong to the American Indian race or whether they are of Mongolian origin. Peary in his work on "The North Pole" [3] holds to the latter view. He says there is a theory first advanced by Sir Clements Markham that the Eskimos are the remnants of the ancient Siberian tribe driven out by the fierce Tartar waves in the Middle Ages. Peary believes that some of the Eskimos are of a distinctly Mongolian type; they display many Oriental characteristics, and there is a strong resemblance between the stone houses and the ruins of the houses found in Siberia. As a general rule, the Eskimos are short in stature, as are the Chinese and Japanese. The women are short and plump and have rather slender legs.

That portion of land between North America and northwestern Europe, which allowed the American Red Indian race and the northwestern European people to communicate and merge, probably was south of the Eskimo's arctic zone; in other words, it may have included

2. Brinton, Daniel G.: The American Race, New York, N. Hodges, 1891.

3. Peary, Robert E.: The North Pole, ed. 2, New York, F. A. Stokes Company, 1910.

Labrador, Newfoundland, Nova Scotia and part of the American continent even farther south.

The view that the Eskimo and American Indian races are distinct does not antagonize but favors the theory of which this paper is the proponent, that of a multiple primate ancestry for races of different color.

A new stimulus was given to the view long held by me of the primate ancestry of the human race by the reading of the address of Sir Arthur Keith,[4] President of the British Association for the Advancement of Sciences, at its annual meeting at Leeds. At this meeting, late in August, 1927, Sir Arthur presented undeniable data which enforced the Darwinian hypothesis.

The address was given a historical setting by his reference to a meeting of the Association also held at Leeds sixty-nine years earlier, that is in 1858. At this meeting Sir Richard Owen attacked the views which Darwin had expressed in various publications. At the time of the meeting at Leeds in 1858, Darwin was engaged on chapters of his book on "The Origin of Species" [5] which appeared fifteen months later (1859). Huxley took up the cudgel for Darwin and wielded it with such effect that, later in 1860, at a memorable meeting at Oxford, he left Owen and the Bishop of Oxford without a leg on which to stand.

Preceding Sir Arthur Keith by four years, a German investigator, Dr. Klaatsch,[6] published the first work of which I have any knowledge on the multiple primate ancestry of man. Exceptions have been taken to some of the views advanced by him, but his conclusions have much in common with those of Keith. Dr. Klaatsch believes that the negro and the Neanderthal man are descended from the same stock that gave rise to the gorilla and the chimpanzee. He also believes that the Mongolians and the northern Europeans have descended from the same stock as the orang-utan.

Recently, another addition has been made to this discussion of the origin of man by Dr. Hrdlicka,[7] an American anthropologist at the Royal Anthropological Society at London. He believes that the existing evidences point to the Neanderthal man as the direct ancestor of ourselves.

Next to "The Origin of Species" one of the most important of Darwin's comparatively early works was "The Variations of Animals

4. Keith, Sir Arthur: Presidential Address Before the British Association for the Advancement of Sciences, August 21, 1927; Cited in the Sunday New York Times, Sept. 4, 1927.

5. Darwin, Charles: Origin of Species, ed. 4, London, John Murray, 1866.

6. Klaatsch, H.: Evolution and the Progress of Mankind, London, K. McKade, 1923.

7. Hrdlicka, A.: Paper Given Before the Royal Anthropological Society, London, Nov. 8, 1927; Cited in the Philadelphia Public Ledger, Nov. 9, 1927.

and Plants Under Domestication,"[8] published in January, 1868. In the preparation of this book Darwin not only spent unusual time and labor in observation and correlation of facts observed by others but also made use of his remarkable powers of generalization in the effort to explain the phenomena of heredity. In doing this he advanced the doctrine or hypothesis of pangenesis which, in a general way, expressed the idea that every cell in plant or animal gave forth gemmules or atoms which exerted a reproducing influence on every cell of succeeding organism, plant or animal. It is worthy of remark that under the modern theory, that atoms are composed of millions of electrons, Darwin's doctrine of pangenesis is more readily comprehensible. The story of pangenesis is only another proof of Darwin's remarkable powers of reasoning on observed facts.

Darwin's "The Descent of Man" [9] was published in 1871, and was followed in 1872 by "The Expression of the Emotions in Man and Animals." [10] Materials collected in these volumes are too well known to need recounting. Darwin died in 1882, and it was not until ten years after his death that Dubois found in the strata laid down by a stream in Java during the later part of the pliocene period the fossil remains of primitive humanity, to which he gave the name of pithecanthropus or ape-man.

As I have already indicated, one of the stumbling blocks to the full acceptance of the Darwinian theory that man belongs to the primates or anthropoid apes is the outworn idea that man is derived from a single ancestral stock. It is clear that he has come from more stock than one, and that these have resulted in the diverse races of different colors—brown, yellow, black and red. Man's descent, in other words, has not been a single file affair.

Special consideration should be given to the question of the origin of the white or Caucasian race. The first types of prehistoric man were probably of a dark color. Primitive man, like his more immediate primate ancestry, was nomadic. He wandered hither and thither in search of food and to escape from and to attack his enemies, animal or human. He fled perhaps to the hills and toward the high regions of the Caucasus, Urals and Himalayas and to the frozen plains of the north.

The changes of color from dark to light, in the thousands or millions of years of prehistoric development of man, was climatic. It was the result of the change in latitude and elevation from the plains. The white

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8. Darwin, Charles: Variations of Animals and Plants Under Domestication, London, John Murray, 1868.

9. Darwin, Charles: The Descent of Man, ed. 1, New York, D. Appleton & Company, 1871.

10. Darwin, Charles: The Expression of Emotion in Man and Animals, ed. 1, New York, D. Appleton & Company, 1873.

or Caucasian race, therefore, may have been an offshoot or an outgrowth of the races of darker color. It was a nordic or mountain race, a race of the cold plateaux and highlands. It is in accordance with this idea that one finds evidences of progressive evolution through the brains of dark colored races, like Chinese and negro, to that of the white or Caucasian race.

The conditions, such as I have described, found in criminals and other brains of low type do not contradict this view. These fissural and gyral aberrations are simply atavistic, both as regards low racial types and animal ancestry. This view of the white or Caucasian race, therefore, upholds the general Darwinian idea of human descent.

The curious fact that blond people are found at various points on both the North and South American continents seems to me to be in accordance with the idea that the light color is of climatic origin, the result of both cold and elevation. Among the Cordilleras of south California and Mexico, blonds occasionally appear amid the generally dark peons who form the bulk of the people.

A year or two ago, a South American explorer brought from the high regions of Brazil a blond family of four or five members of which an interesting account was given in the newspapers and magazines. A blond or light colored native race is found today on the pampas of Argentine. The Andean heights, which separate Argentine from Chile, reach tremendous elevations of 18,000 feet or more. Presumably the blond natives of the pampas were original inhabitants of some of the higher elevated Andean plateaux.

As ages progressed, the white race became not only a nordic and mountain people but a conquering and controlling race—in its subdivisions represented by the Goths and Visigoths, by the Vikings and Vandals, and by the followers of Alleric and Genghis Khan. As civilization has spread and advanced, all races and colors have commingled for the benefit of all.

Different races have appeared at different times and places more or less separated. The illustrations of this fact are slowly being accumulated. We have Dawson's account of the remains, found in 1912, at Piltdown not more than thirty miles from the home of Darwin, and the remains of types of anthropoids with manlike pointings found in India, Thibet, various parts of Europe and in America. All of these seem to indicate that the transition from apes, like the gibbon, the chimpanzee, the orang, and the gorilla to primitive man, began during the miocene period, at least 600,000 or perhaps 1,000,000 years ago.

Numerous well established facts confirm the relationship of man with the anthropoids. Examination of the blood of anthropoids and of man has shown the same constituents. Bacteriologists have demonstrated that the anthropoid apes are subject to the same infections as man.

Anthropoids and man are so alike in their structural organizations that experimental physiologists transfer the results of their investigations of the brain of one to the other. The same embryologic conditions and actions occur in the womb of the anthropoids and of the human being. To use the words of Sir Arthur Keith, "We find the same vestigial structures—the same 'evolutionary post-marks' in the bodies of man and anthropoid. The anthropoid mother fondles, nurses and suckles her young in the human manner."

Darwin, who was not a professional anatomist, accepted the opinion of Huxley that "The human brain was but a richly annotated edition of the simpler and older anthropoid book, and that this edition, in turn, was but the expanded issue of the still older primate publications."

Since the time of Darwin and Huxley, thousands of anatomists and physiologists have studied and compared the brain of anthropoids with that of man. Like Elliot Smith, they have come to the conclusion that no structure that exists in one is absent in the other. The differences between the brain of anthropoids and that of man are quantitative rather than qualitative. They are differences of degree rather than of kind.

The brains of all mammals, including of course, the anthropoids as pointed out by Sir E. Ray Lankester, have increased in size and therefore probably in capacity. The human brain, more than that of any other mammal, has gained in size and endowment.

In order to show that my views on Darwinism and evolution are not the result purely of reading but are based on actual observation and investigation, I shall refer to some of my early personal published work bearing on this question. After my appointment as neurologist to the Philadelphia Hospital, in 1877, I soon had unusual opportunities to study human brains, and the macroscopic examination of the brain became almost a passion with me. Not only was I well informed as to the actual correlatives of the ventricles and horns, but there was scarcely a fissure or gyre, large or small, with which I was not familiar. I noted closely the morphologic and anatomic differences in the configuration of different brains, and these I found to be great in the brains of criminals, defectives, idiots and paranoiac persons.

My presidential address [11] at the annual meeting of the American Neurological Association, in 1886, was largely given up to a discussion and description of arrested and aberrant fissures and gyres in the brains of criminals, idiots, negroes and insane persons.

Passing by the question of disease in the numerous specimens of brain studied by me, I wish to emphasize now, as at the time of the first

11. Mills, Charles K.: Arrested and Aberrant Development of Fissures and Gyres in the Brains of Paranoiacs, Criminals, Idiots, and Negroes: Preliminary Study of a Chinese Brain with Andrew J. Parker, J. Nerv. & Ment. Dis., 1886, vol. 13.

appearance of my papers on this subject, only the morphologic and anatomic differences and similarities among these human brains and the brains of primates. Expressed briefly, the human brains all had simian appearances and characteristics and the simian brains all had striking similarities with the low type human brains. I made a careful comparison of the human with the anthropoid brains pictured in Gratiolet's atlas of primate brains.

In the anthropoid brains, the fissures were sometimes longer and deeper than in the human brain. The human brains, on the other hand, were sometimes bridged by the *plis de passage* described by Gratiolet in anthropoid brains. The convolutions in the simian brains were less voluminous and simpler in type than those in the human brain, although in some instances the two approached each other closely.

My studies in racial types, Asiatic, African and European, uphold the view that the origin of the human race is not from a single ancestral line, but, like that of the anthropoids themselves, is from distinct and separated strains. As Sir Arthur Keith puts it, the development of the human race from the anthropoids pursued a zig-zag and irregular course instead of being in a straight line from one ancestral stem.

Why all the anthropoids failed to reach the human status but remained in their original state or became more degenerate is not within the scope of these remarks.

I might bring numerous other facts based on my own personal investigations to uphold the Darwinian idea of the descent of man as taught by Sir Arthur Keith. By the effects of electrization (faradization) of different regions of the brain during operations, I have shown more than once how the cerebral motor area is separated from the areas concerned with various forms of sensation in man as well as in the anthropoids.[12]

When Professor Sherrington was in this country, in 1904, delivering a course of lectures at Yale University, he came to Philadelphia on my invitation and gave an illustrated lecture in the medical laboratories of the University of Pennsylvania in which he showed the results arrived at by Grünbaum and himself through faradization of different cortical areas of the brain of a gorilla. These experiments demonstrated among other things that the motor area in the brain of the gorilla was almost entirely cephalad of the central fissure, and that this area was much subdivided into special centers. Some years before this time (in 1888), as a result of personal observations made by necropsies or during operations, I had arrived at the same conclusion with regard to the human

12. Mills, Charles K.: The Motor Area of the Human Cerebrum, Its Position and Subdivisions, with Some Discussion of the Surgery of the Motor Region (with Dr. C. H. Frazier), Univ. Penn. M. Bull., July and August, 1905.

brain, namely, that the motor region was in front and the sensory areas behind the central fissure.[13]

Sir Arthur Keith indicates that one must look into the details of the selection which goes on in the development of the human brain to comprehend the subject of its evolution. In every respect this study points to the correctness of the Darwinian opinion. Going further, he furnishes additional evidence favoring the idea of evolution.

Sir Arthur directs attention to the influence of the sex glands on the growth and development of the human brain. He refers to Starling's great law of hormones, which are missives sent from one of the glands of internal secretion to bring about a harmony of action with other glands for the benefit of the entire body, but I leave this part of the discussion to Dr. Dercum. Darwin, Sir Arthur declares, would have welcomed this discovery, as it would have given him "a rational explanation for so many of his unsolved puzzles including that of 'correlated variations.'"

One thing that has proved somewhat disturbing to the evolutionist who believes that man has descended from a primate ancestry is the fact that primates of intermediate types, such as the baboon and gibbon, or the higher anthropoids, such as the orang-utan, chimpanzee and gorilla as they are now found in the wilds of Africa, the Malayan Islands or elsewhere, show little if any evidence of man's kinship to them.

Tilney, in the announcement of his great biologic work "The Brain from Ape to Man,"[14] truthfully says that: "To consider any of the living apes the possible ancestor of man is an inconsequential, trifling and incomplete view of the situation which requires a much more extensive understanding of the biological process for the complete appreciation of its significance. It does not seem sufficient to linger among the modern apes in search of our ancestors. These animals belong to families totally divergent from man. . . ."

A larger consideration must be given to the subject in order properly to understand the steps by which man emerged from a primate ancestry. The retrospect must take in at least a million years of evolution, and at the same time must not disregard Hughlings Jackson's theory of dissolution which can be applied not only to man, but to animals lower than man in the scale of development. After prehistoric man had appeared on the scene in widely distributed parts of the earth and after man was well on his way in the process of higher evolution, it is probable that the

13. Mills, Charles K.: Cerebral Localization in Its Practical Relations, Tr. Congress Am. Phys. & Surg., Washington, D. C., vol. 1, 1888 and Brain, July, 1889, vol. 12.

14. Tilney, Frederick: The Brain from Ape to Man, New York, Paul B. Hoeber, 1928.

primates most nearly approaching man in stature and other physical conditions passed into a period of primate dark ages.

The gorilla, for instance, is found today in largest numbers near the western coast of the mid-African region and in the Kivu plateau region of the interior. Here he roams unmolested over a region a hundred or more miles in extent, at odds with the dwarf-men who are his most immediate neighbors. As far as can be determined, his age of darkness is increasing rather than diminishing. After all, however, supposing the truths to be as just surmised, the facts are not a valid objection to man having reached his human estate by way of the higher primates.

The history of man's progress has afforded well known illustrations of his tendency to relapse into comparative barbarism for a time, this to be succeeded by a renaissance and further advances toward a more perfect state. I believe with Tilney, as regards the human brain, that man is only in a certain stage in the upward process of development.

It may perhaps be futile or foolish to theorize on the method by which the transition from the primate to prehistoric man took place, and yet such speculation has its attractions and may not be without value in the consideration of the question of the descent of man. The first steps taken in primate development of man from the lower animals were probably shown in the use of language and of the limbs. With respect to the latter, it may be said that the anthropoid or intermediate primate began to use the terrain, and found he could do this best by employing his hind limbs for station and progression and his fore limbs for prehension and combat. It must be remembered that only a portion of the intermediate and higher primates separated themselves from the general stock and came to play a higher rôle.

With regard to the linguistic phase of evolution, the first step in the prehistoric development was the use of language, largely emotional or instinctive, just as in the human being of today emotional and instinctive speech is the first to come as it is the last to go. The primate parents made use of certain sounds to call to their children, and the latter sought out their parents by the use of similar sounds. Feeling of fear and hate, on the one hand, and of satisfaction or joy, on the other, resulted in an ejaculatory or interjectional speech. Such emotional language became more or less fixed for the same animal group, and thus language was started.

Later in the history of the advancing primate, reason began to play its linguistic part. The animal recalled the sounds made by its foes or by destructive agencies like the wind and flood, and in imitation and in response made use of sounds which became additions to the language.

Words at first merely represented the concrete: rock, trees, birds of the air, beasts of the field, sun, moon and stars. Later, other sounds became necessary to express actions of the things seen and heard; thus,

step by step, a simple form of language developed, and this language, simple and imperfect as it was, marked an important step in human evolution.

As might be expected, written language at first was largely pictorial, as illustrated by the rough representations on the rocks which have been traced to the neolithic times.

Some believers in evolution seem to be worried by the idea that man's descent should be traced through anthropoids and the intermediate primates. Apparently, according to some, man himself was an order set apart, although there is not a particle of evidence in favor of this supposition. The truth would seem to be that man, after he started on his prehistoric journey, more or less rapidly fell away from his primate progenitors and has continued to do this through the ages down to the present.

Some one has said that speech, the ability to talk, is what separates man from the lower animals. As matters stand in the world today this may be true, but in the early stages of human evolution developing speech formed one of the links that joined man with his primate forebears. In his methods of worship, in his control of the family, in following the chase or war, names became useful or necessary to prehistoric man. These names, as I have already said, first sprang from emotions like affection, fear and joy. Undoubtedly these also originated from the necessity to indicate parts of himself and also the world which surrounded him.

Brinton, well qualified for the actual task, made a careful study of the dialects of the many tribes or nations of Mexico, Central and South America. Important studies of the dialects of tribes of North America have been made by other well known American archeologists. Much in these dialects served to link the American race together and served also to separate it from the races with other primate ancestry. These studies embraced not only the vocabularies of these tribes, but the methods of inflecting and vocalizing them; in other words, what might be termed their primitive grammar and methods of pronunciation.

Of course, since the coming to the American continent of the Spanish, Portuguese, English, French, Dutch and other European people, the original Indian dialects may have undergone changes due to European admixture; but since these linguistic studies of Brinton and others were begun, there have been regions in North and South America where the native dialects continued to be spoken in an unadulterated form.

As I have said, one of the first steps taken in the development of man from the higher primates was in the use of language. An experiment made some years ago in the psychologic department of the University of Pennsylvania is interesting in this connection. Professor Lightner Whitmer, Professor Fernberger and other members of the

psychologic staff tried what they could do in teaching a chimpanzee to speak as well as to perform other purposive acts. After numerous attempts, the chimpanzee learned to say with lip movements a simple word such as "mama."

In the course of time, Professor Fernberger told me that they were able to recognize five distinct vowel sounds used by the chimpanzee in response to certain emotional stimuli. These were cries of modulated sounds which were used to express terror, fear, sex instinct, feelings of pleasure on the one hand, and of annoyance on the other. It would be childish to believe that in a few short weeks or months language could be acquired by an anthropoid, but the fact that in this chimpanzee a form of responsive speech could be brought out by training seems to make it not improbable that some members of the present anthropoid stock might have acquired the same form of language.

Recently, in a current periodical, I saw Professor Osborne cited as one who was doubtful of man's close relationship to the anthropoid apes. Professor Osborne, if I have read his contributions to scientific literature correctly, has really done much to link man with anthropoid ancestry, to show that the anthropoid is at least his second cousin. Like him, we all feel that much remains to be done in order to complete the chain by which we have arrived at the Darwinian idea of man's descent. I see no reason why we should not face honestly the question of man's origin, through a process of evolution. To my mind, to think of man as the final outcome of evolutional development is a higher and nobler concept than to believe in a fiat or out-of-hand creation.

The question of evolution and religion is one that apparently cannot be downed. Whatever its origin, there seems to be a widespread opinion in the community that men of science who believe in evolution are irreligious. The foundation of such beliefs are several-fold. In the first place, they are largely the results of ignorance on the part of those who hold them and who are not trained to comprehend the facts of science and hence must form their opinions through the teaching of others. Too often the only instruction they receive bearing on the points in question is of the clerical sort. The word evolution, for some strange reason, seems to arouse in many churchmen the same effect as the waving of the red flag before a bull.

In the second place, the opinion that scientific men, and especially evolutionists, are irreligious is dependent on a false idea of what religion really is. Every one is incurably religious and this is due, in large part at least, to the fact that human nature is two-fold. Men and women the world over are both emotional and intellectual, and emotion or instinct plays a larger part than the intellect in most lives. To use a common form of simile, man is a creature of the heart as well as of the head.

Religion with scientific men is not a matter of creed or ceremony. They believe that creeds are man-made, and in not a few instances are based on pagan superstition.

Heresy is not irreligion. Creeds are not Christianity; they have led to most of the horrors for which Christianity has been held responsible.

So great is the untoward influence of creeds on real Christianity or real religion of any sort that great thinkers like Franklin have advocated the adoption of some sort of creed to which men of all sects and opinions could agree.

With regard to the subject of evolution, it is a healthful sign of the times when an American bishop and American professors and students in theological seminaries and when an English bishop and dean are inclined to support the Darwinian theory of evolution. Kings and archbishops, religious synods and assemblies may fulminate against those who teach evolution, but the truth has found its way through the crust of conservatism.

In conclusion, I believe with Cicero and Seneca that age has its advantages and compensations as well as its trials and vexations. One of these advantages lies in the fact that the man in advanced years can look back and balance mentally the results of his experience. This is especially true when one's long life has been spent in the period of the world's history in which science, art and literature have made their greatest advances. When I look back through the more than forty years of the history of the Philadelphia Neurological Society, the survey of what has happened in this time fills my heart with thankfulness. The doctrine of evolution sixty years ago, largely decried and frowned on by society and the church, is today accepted openly by a constantly increasing number.

Finally, why all this ado about chance, design and first cause, about Paley and Huxley and all the rest? God lives with us, forever revealed in the laws of His universe. Man sprang not into being full panoplied like Minerva from the brain of Jove, but came to this earth as the sum and summit of ages of evolution as taught by Charles Darwin, the greatest of modern sages and scientists.

PHYSIOLOGY, PSYCHIATRY AND THE INHIBITIONS*

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The relation of neurophysiology to psychiatry is obviously too close and too extensive to be discussed adequately in one lecture. I must content myself with indicating the field in general, and with taking up in particular some one phase of the relationship. To begin with, let me ask how closely physiology and psychiatry really approach each other? Figure 1 diagrammatically illustrates their connection. Such a structure as the science of psychiatry should be based more and more on the better known, the more "exact," sciences. Anatomy, with some help from physics and chemistry, may be indicated as the main support. Built on these, there is a considerable block of physiology; just above comes the more complex but similar science, experimental psychology. These studies are all "close to the ground" and are well founded on controllable observations. If one wishes to build higher, to things mental, one finds just above these fundamentals a great hiatus—a void in present knowledge that makes the mental sciences difficult to control. Psychiatry has accumulated a great many data by observation. The psychiatrist knows much about what occurs in abnormal minds, but little about why and how it occurs. Analytic psychology, though of recent birth, also has accumulated a mass of facts. These are even less controllable than the facts of general psychiatry, for the very method of analysis has the inherent weakness of being partly subjective, i.e., dependent on the personal equations of two persons—the analyzer and the analyzed. The still more abstract theoretical psychology comes next, leading to the most abstract and universal of all subjects, philosophy, at the apex (see fig. 1).

But to return to the void in the center: It is here that research must be carried on for many years in order that block after block of facts may be built up to explain the observations and to support satisfactorily the theories now promulgated in psychiatry and in psychology. Many of these theories are no doubt correct. Indeed, correct explanations are often discovered years before proof is available. Charcot, Freud, Janet, Prince and many others are probably right, but at present their theories have little support in scientifically controlled observations. They can present practically no data that would be accepted by a physicist or even by a physiologist as sufficiently controlled. Probably one has no right to expect such proof at present. The data of physics are so different from those of psychiatry that similar methods cannot be expected, but that

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*A lecture delivered at Yale University, Jan. 27, 1927.

is no reason for saying that accurate, controllable methods never can be applied to the observation of human behavior. It is perhaps because the science of psychiatry is young that it seems too complex for scientific "quantitative analysis." All sciences pass through an infancy in which "qualitative analysis" [1] alone is possible. Obviously, then, there is room for all to work at filling the void. From above, a careful accumulation of

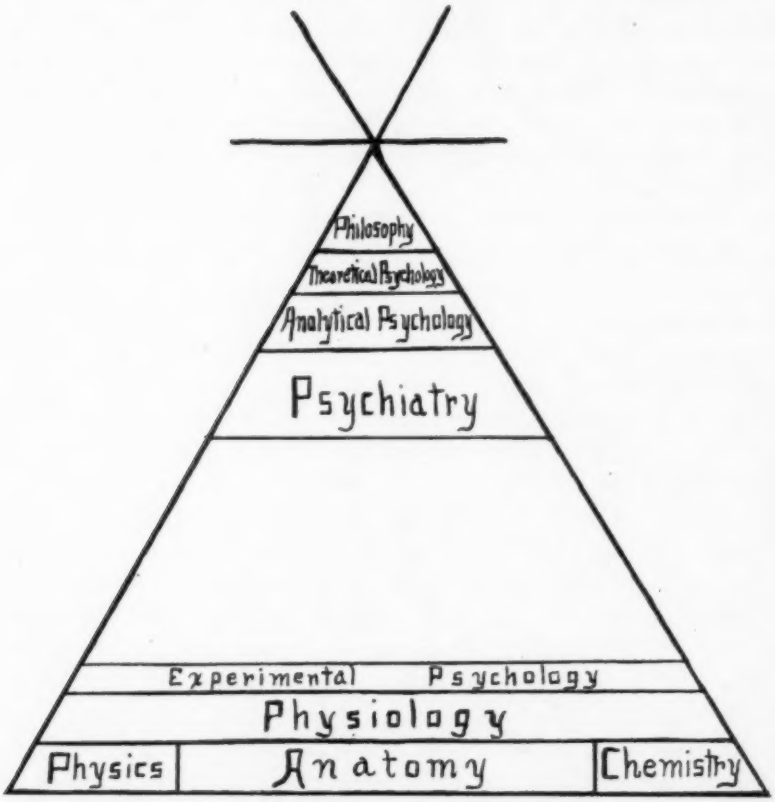

Fig. 1.—Diagrammatic formulation of the relationship between physiology and psychiatry. The different divisions are arranged according to degree of objectivity, from below upward. Similar pyramids could be built for other subjects, each with its apex joining the central correlator—philosophy.

objective observations will be of permanent value; from below, one must build upward on the basis of anatomy and physiology in order to elucidate the mechanisms of the increasing data of psychiatry.

1. These terms "quantitative" and "qualitative" are borrowed from the realm of economics, as discussed by Cobb in a recent paper. (Am. Econ. Rev. **16**:424, 1926.)

Already much progress is being made. Sherrington's [2] work gave the start that will carry the psychiatrist far into the unknown regions. Adrian,[3] Forbes,[4] Kato [5] and Davis [6] added new methods and fundamental observations concerning the nature of the nerve impulse and reflex integration. Coghill [7] and Detwiler,[8] using the technic of anatomy, have shown the relation of reflex patterns to embryologic development. Pawlow [9] invaded the sphere of cerebral physiology and devised methods which give consistent and reliable results, although they deal with "mental processes." Though his experimental data are published largely in Russian and are therefore little known as yet, his researches have greatly influenced psychology. Finally, Cannon [10] in his studies of the emotions with their endocrine and sympathetic connections has given the psychiatrist insight into the relationship between "mind and body." One might mention numerous other investigators who have laid stones to build this level of the pyramid, but these suffice to point the way.

INHIBITION

As an example of the complex relation between physiology and psychiatry one might discuss "inhibition." It is a much abused term, for it is used physiologically, psychologically and vulgarly. It is used much and loosely. According to the dictionary, to inhibit means to check or to bridle.

Physiologic inhibition was first discovered by Weber, in 1846, when he observed the slowing of the heart on stimulation of the peripheral end of the cut vagus. A great deal of work has been done since then to explain the mechanism of this phenomenon,[11] but no thoroughly satisfactory explanation has yet been reached. It is certain that vagal nerve impulses set up an altered physicochemical state in the

2. Sherrington, C. S.: The Integrative Action of the Nervous System, New Haven, 1906. Fulton, J. F.: Muscular Contraction and the Reflex Control of Movement, Baltimore, Williams & Wilkins Company, 1926.

3. Adrian, E. D.: J. Physiol. **61**:49, 1926.

4. Forbes, A.: Physiol. Rev. **2**:361, 1922.

5. Kato, G.: Nankodo, Tokyo, 1926.

6. Davis, H.: Physiol. Rev. **6**:547, 1926.

7. Coghill, G. E.: J. Comp. Neurol. **42**:1, 1926.

8. Detwiler, S. R., and Lewis, R. W.: J. Comp. Neurol. **39**:291, 1925.

9. Pawlow, I.: Twenty Years of Objective Study of Higher Nervous Activity (Behavior) of Animals, State Pub. Bureau, Leningrad, 1925. Evans, C. L.: Recent Advances in Physiology, Philadelphia, 1926.

10. Cannon, W. B.: Am. J. Physiol. **76**:326, 1926.

11. Some of the principal contributors are Gaskell (1886), Howell (1905), Loewi (1921), Brinkman (1925) and Witanoski (1925). Recent reviews are given by Howell (Physiol. Rev. **5**:161, 1925), Adrian (Brain **47**:399, 1924) and Fulton (Muscular Contraction and the Reflex Control of Movement, Baltimore, Williams & Wilkins Company, 1926).

heart muscle; in fact, they probably cause the formation of an inhibitory substance (fig. 2). This substance resembles in its action a potassium solution. Indeed, by perfusing two hearts in series and inhibiting one by vagal stimulation, the second heart is also shortly inhibited. Moreover, this perfusing fluid produces the characteristic effects of vagus stimulation on other organs, i.e., if the stomach is connected with the perfused heart, it contracts (fig. 3).

There is thus presented an example of simple, direct inhibition on a peripheral muscular tissue by a peripheral nerve. It is known that there are inhibitory nerves which act directly on the smooth muscle of the intestines, viscera and blood vessels. Glands also may have direct inhibitary nerves. But these tissues have a certain degree of automatism: heart muscle and smooth muscle may contract rhythmically when entirely separated from the nervous system. Skeletal muscle is different; as Sherrington [12] said, "section of its nerve, even when the muscle remains in situ, causes it to lapse into a paralytic quietude so profound that its very structure becomes in a short time hardly recognizable as muscle! Its contractive function and its very nutrition are indissolubly dependent on the neuraxial center which innervates it." Consequently, the vertebrate skeletal muscle does not need a peripherally acting inhibitor; mere abolition of nerve impulses to a muscle will cause its complete relaxation. Moreover, there is only one path by which impulses from the neuraxial center may reach the muscle, namely, the peripheral nerve, which is made up of axons from the ventral horn cells of the cord. Many motor pathways converge on these cells. Impulses from the dorsal-root ganglion cells via the short cord tracts have their influence; from higher up (to enumerate some of the principal paths) the vestibulospinal tract, the reticulospinal tract, the rubrospinal tract and, finally, the corticospinal tract play on the ventral horn cells. For all these there is but one effective end-organ, the muscle. Through it alone they externalize their nervous energy by causing muscular contraction and thus action in respect to the environment. Hence, the peripheral nerve must be the "final common path" (to use Sherrington's apt term) for all motor impulses.

A phenomenon first observed by Wedensky, in 1885, is interesting in this connection. He observed in a partially fatigued nerve-muscle preparation that electrical stimuli of a certain strength and frequency applied to the nerve caused in the muscle only a slight initial twitch, followed by relaxation. During such stimulation, additional electrodes applied to the nerve nearer the muscle were unable to cause contraction. Less frequent stimuli gave no such effect. Years later, the experiments

12. Sherrington, C. S.: Quart. J. Exper. Physiol. **6**:282, 1913.

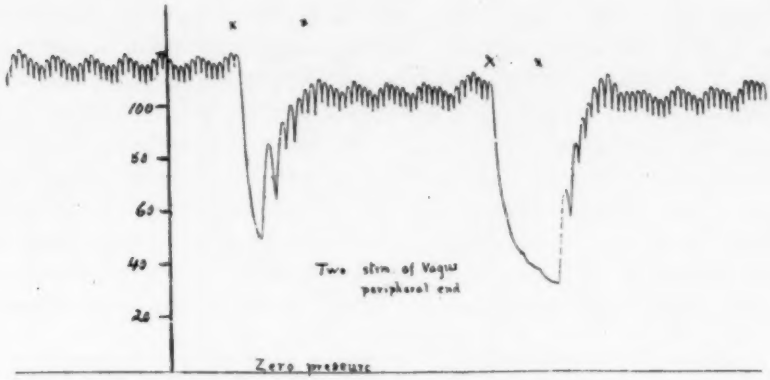

Fig. 2.—Inhibition of the heart in the dog. Between x—x, the peripheral end of the cut vagus is stimulated. (After Howell: Textbook of Physiology, 1924.)

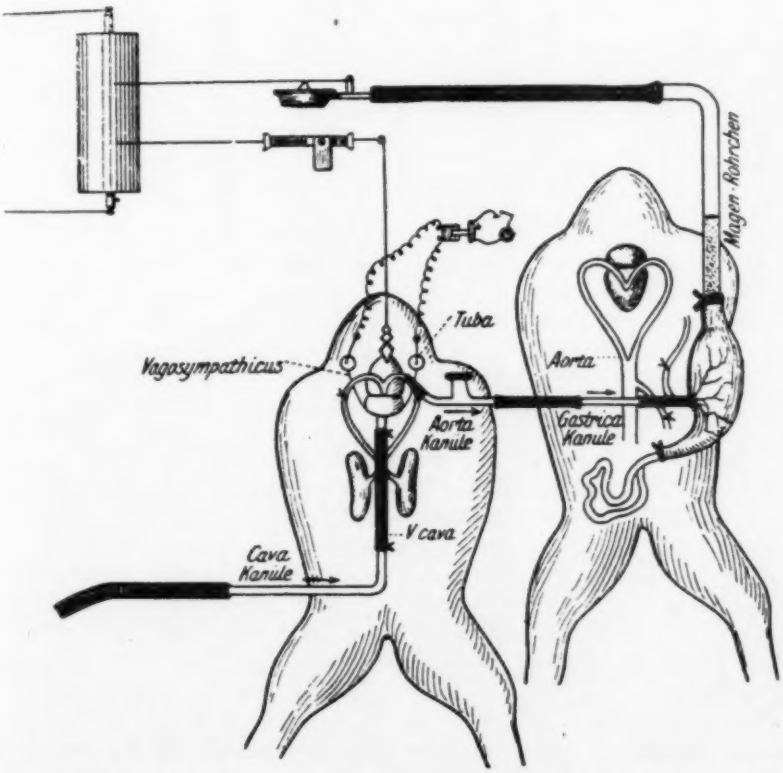

Fig. 3.—Humoral conduction of the products of nerve stimulation from the heart to the stomach (after Brinkman: Arch. f. d. ges. Physiol. **196**:75, 1922). Stimulation of the vagus causes inhibition of the heart of the first frog, but the perfusion fluid passing through this inhibited heart when perfused through the stomach of another frog causes the stomach to contract.

of Lucas [13] on nerve conduction explained this "Wedensky inhibition." He showed that a nerve impulse passing along a nerve fiber leaves in its wake a refractory period, which may block or make subnormal a too closely following impulse. As impulses are known to come in groups or "streams," they usually do thus interfere with one another and by each one falling in the relative refractory phase of the one preceding, a subnormal response usually results (fig. 4). If the neuromuscular mechanism is then fatigued, "Wedensky inhibition" is produced. Applying such observations to the theory of reflex inhibition, Lucas, Adrian and Forbes [4] have ingeniously worked out the "interference" explanation of

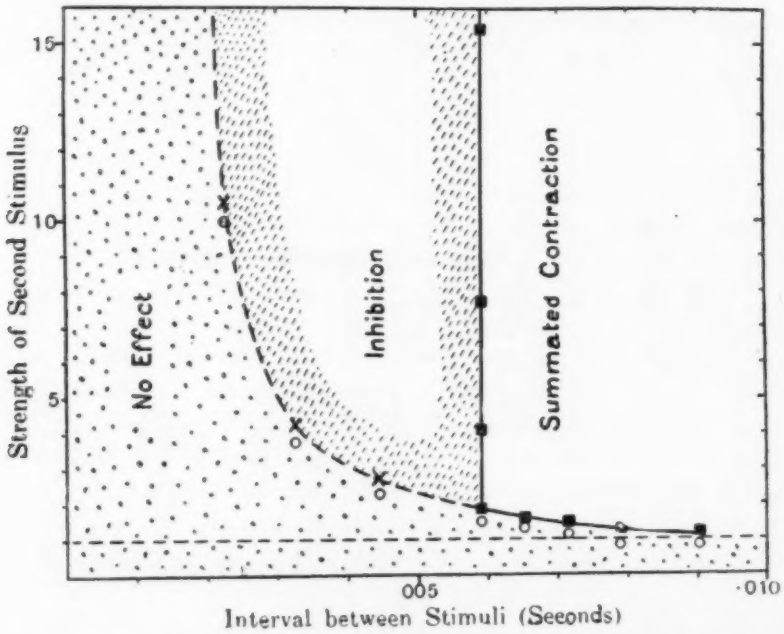

Fig. 4.—In this experiment of Adrian's (J. Physiol. **46**:400, 1913) weak stimuli to the nerve of a nerve-muscle preparation following one another at an interval of 0.006 second or more cause summation of contraction (black squares), whereas stronger stimuli following each other more closely either cause inhibition (crosses) or have no effect (circles). In this preparation, for example, the nerve was in a complete refractory period for 0.0025 second after stimulation, and was in a relative refractory period from 0.0025 to 0.006 second after the first stimulation.

central inhibition. (In discussing inhibition as viewed by the fundamental sciences, I wish to give space only to experimental observations, so this theory is briefly described in the legend to figure 5).

13. Lucas, K.: The Conduction of the Nervous Impulse, London, Longmans Green & Co., 1917.

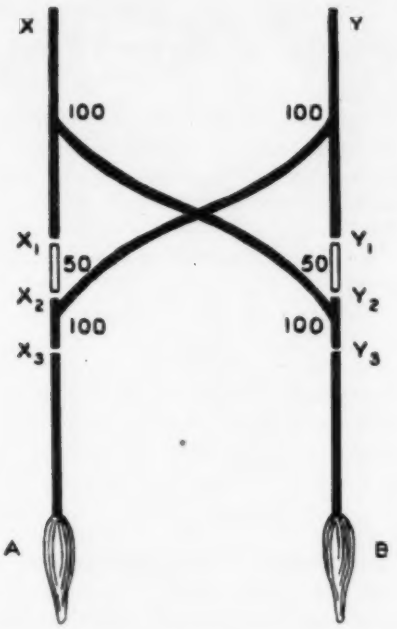

Fig. 5.—Diagram illustrating the interference theory of inhibition, after Lucas (The Conduction of the Nervous Impulse, London, Longmans Green & Co., 1917).

X is a reflex pathway to muscle A with four relays (x, x_1, x_2 and x_3) separated by three synapses. X is the afferent nerve, from X to x_3 is a nerve center, while x_3 is the motor nerve to A. Y-B is a similar neuromuscular system. Each gives to the other one collateral, joining the third motor relay. It is supposed that paths X and Y can each conduct 100 impulses per second without much reduction due to refractory periods; path $x_1 x_2$, however, has a long refractory period so that every other impulse that comes from X falls in a refractory phase. This path $x_1 x_2$, therefore, has a rate of 50 impulses per second, and 50 is optimum for path $x_2 x_3$ which has such a rate of recovery that if the frequency of the impulses is 50 per second each falls in the phase of supernormal recovery, and is thus large enough to pass the synapse at x_3. But if Y is stimulated, 100 impulses per second come to x_2 and cause all impulses from x_1 to fall in the refractory phases, so that none are large enough to pass x_3 to muscle A. Thus, when X is stimulated, muscle A contracts and muscle B is inhibited, whereas when Y is stimulated the reverse takes place. Certain objections to this theory have recently been advanced by Fulton (Muscular Contraction and the Reflex Control of Movement, Baltimore, 1926) and Samojloff (International Physiol. Congress, Stockholm, Abstracts, 1926, p. 145).

All available evidence indicates that inhibition of skeletal muscle takes place centrally.[14] This inhibition is a fundamental function of the central nervous system; without it reciprocal innervation would be impossible, for the effectiveness of all simple reflexes depends on the relaxation of antagonists, to allow the prime mover to act. Forbes [4] gives the elementary facts as follows:

"The normal dominant reflex responses to stimulation of an afferent nerve in a hind limb are the flexion reflex and the crossed extension reflex; the flexion reflex consists in reflex excitation of the flexor muscles and inhibition of the extensor muscles in the same limb as the stimulated nerve; the crossed extension reflex consists in reflex excitation of the extensors and inhibition of the flexors in the opposite hind limb. Under certain conditions some of these responses may be replaced by their exact opposites."

Furthermore:

. . . "if the strength of the stimuli applied to the afferent nerve in the opposite leg were chosen with sufficient care a partial inhibition could be demonstrated; that is, combined stimulation produced a degree of muscular contraction intermediate between the full contraction induced by crossed stimulation alone and the total relaxation induced by stimulation of the ipsilateral nerve alone."

Finally:

. . . "there is the intrinsic tendency of the spinal centers to exhibit *rhythmic alternation* between flexion and extension, under some circumstances, even in absence of afferent stimulation. For instance, Sherrington [15] showed that the scratch reflex proceeds with unaltered rhythm in response to steady cutaneous stimulation of a remote portion of the body when all afferent fibers from the muscles themselves have been cut."

Such observations as these of Sherrington, Lucas, Forbes and others show that a selective process takes place in the cord near the ventral horn cells. There is competition for the final common path: impulses from one tract reach the ventral horn at one moment; other impulses are inhibited and still others facilitated; immediately, changes and reversals take place. With proper integration, the result is reflex coordination and normal behavior. Just how this is accomplished is not known, but obviously central inhibition plays a large part. Lucas' theory of "interference" has been mentioned; more recently, Sherrington [16] has promulgated a "chemical" or "humoral" theory (fig. 6), which has certain interesting analogies to the mechanism of cardiac inhibition. (Incidentally, in a prolific and epoch making career of forty years, this is the first theoretical paper Sherrington has published!) But both of these theories are speculative, and much remains to be learned by objective experiment before they can be incorporated as a part of the data of physiology.

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14. Adrian, E. D.: Brain **47**:399, 1924.
15. Sherrington (footnote 2, first reference).
16. Sherrington, C. S.: Proc. Roy. Soc. B **97**:519, 1925.

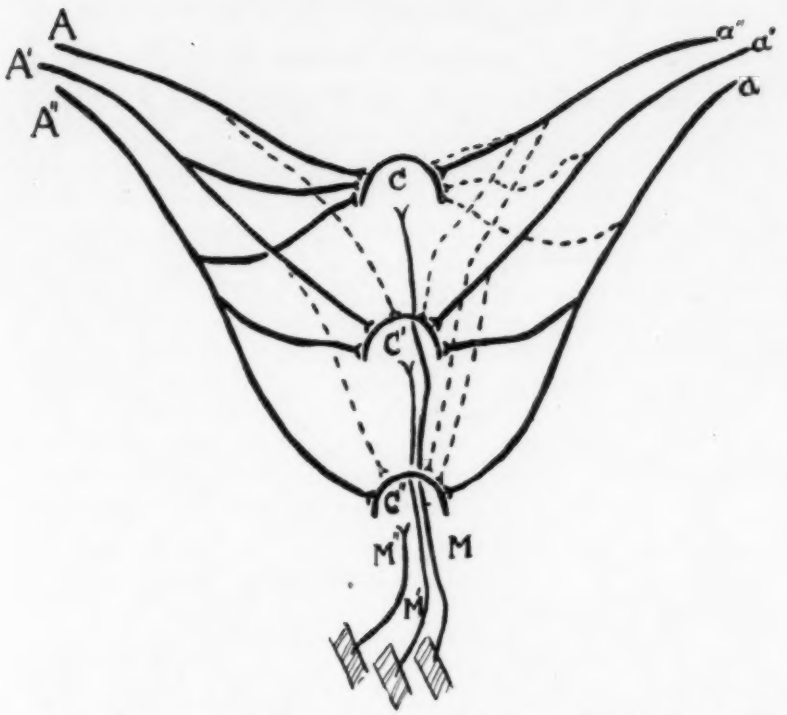

Fig. 6.—The discontinuous, refractory, all-or-none character of the nerve impulses in the afferent fibers (A) and in the motor neurons (M) is accepted as in the "interference" theory. It is the process in the nerve centers (c) that is considered nonrefractory and thus essentially different. Here a certain quantity of stimulating agent E is supposed to accumulate whenever an afferent impulse arrives; E is quickly dissipated, but the quantum formed by one nerve impulse may be added to by other quanta caused by other nerve impulses arriving by the same or by different routes (A, A', A'', etc.). The different centers (C, C' and C'') have different thresholds, and the direct paths (A-C, A'-C', etc.) have more ability to develop E than their collaterals (A-C', A-C'', etc.). With these postulates Sherrington can easily explain such reflex phenomena as after-discharge, summation and reinforcement; he also takes up many more complex observations and explains them satisfactorily. Thus one has a "chemical" view of the excitation process; the substance E, which is theoretically formed in the centers, is a product of nerve stimulation. This chemical theory is not without experimental confirmation in the central nervous system, e. g., the retina may receive brief strong light stimuli which give prolonged after-images. In other words, a physico-chemical process of long duration may be set up by extremely short stimuli (Fulton: Muscular Contraction and the Reflex Control of Movement, Baltimore, 1926).

The chemical view of inhibition is similar; the production of an inhibitory substance (I) is postulated at some point in the reflex arc. Thus, one has added to the excitation schema inhibitory paths (a, a', a'') each going mainly to one center, but connecting to a less extent via collaterals with the other two (a-c' a-c'', a''-c, etc.). Impulses along these paths cause the formation in the centers (c, c', c'') of "substance I," which inhibits muscular contraction by neutralizing "substance E." Various combinations of A, A' or A'' with a, a' or a'' playing on c, c' or c'', when given rough quantitative values, appear to explain adequately the various experimental observations on spinal reflexes.

STUDIES OF PAWLOW

When dealing with the highest centers of integration it is difficult not to employ psychologic terms that are confusing to clear thinking. Pawlow [17] has emphasized this and succeeded remarkably in making objective the study of cerebral activity. His division of reflexes into unconditioned (inborn, instinctive) and conditioned (acquired, associated) is now too well known to need description. In the course of his experiments on dogs with salivary fistulas, he made such observations as these:

A dog is trained to expect food five seconds after hearing a bell; salivation then begins three seconds after he hears the bell. But a change is made, and food is not given until three minutes after the

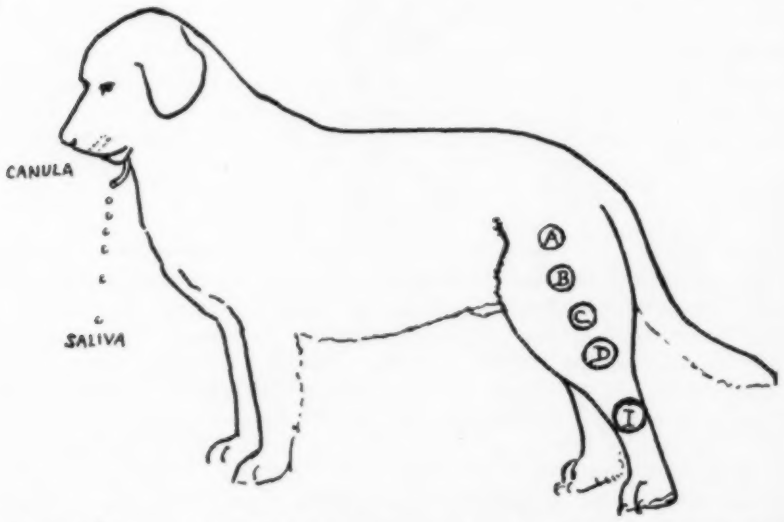

Fig. 7.—Diagram drawn by the author to illustrate Pawlow's experiment as described in his Lecture XIX, referred to in text.

bell. After a few trials the salivation, which used to come in three seconds, is entirely held up—inhibited—and does not appear until just before the three minute period has elapsed. This form of inhibition is known as "delay." [18] A simpler form is "eclipse," in which the secretion is inhibited because, after the animal is trained with bell and food, as described, the food is omitted entirely in a series of experiments. That this is not mere disappearance of the reflex is proved by letting the animal rest over night; in the morning, salivation will again appear when the bell is rung.

17. Footnote 9. Skandin. Arch. f. Physiol. **44**:236, 1923.

18. Pawlow (footnote 9, first reference).

If now a mechanical stimulus is given to the skin at the time that the bell is rung, and food is withheld after this combination but is given as usual when the bell alone stimulates, there is inhibition of salivation from the combined stimuli. The conditioned reflex has been conditioned and has caused inhibition. Hence, this may be called conditioned inhibition.

Still another form of inhibition may be shown by combining a skin stimulation with feeding, and thus eliciting the usual salivary secretion. After this has been well learned for one spot, other spots on the skin, a few centimeters distant, are stimulated similarly. At first they cause the secretion, but when food is withheld in all cases except when the stimulus is at the original point, the animal learns to discriminate, and inhibits all salivation except for stimulation at this point. This is called differential inhibition. These four—eclipse, delay, conditioned and differential inhibition—form Pawlow's "internal inhibition."

An especially interesting example of this last type is given by Pawlow [18] in his Lecture XIX. Here a dog has five points of mechanical

Pawlow's Differential Inhibition Experiment

	Number of Drops of Saliva in 30 Seconds			
Second stimulation at.....	A	B	C	D
Waiting 30 seconds after "I" stimulated.....	0	0	0	0
Waiting 1 minute after "I" stimulated.....	5	3	1	0
Waiting 2 minutes after "I" stimulated.....	10	8	5	2
Waiting 3 or 4 minutes after "I" stimulated.....	10	10	10	4
Waiting 5 or 6 minutes after "I" stimulated.....	10	10	10	10

stimulation arranged on his hind leg (fig. 7). From above downward, these are designated as *A, B, C, D* and *I*. At each of the first four points mechanical stimuli are given and followed by food; conditioning takes place and the dog secretes ten drops of saliva in thirty seconds for any of these points. After these reactions are well established stimulation is given at *I*, but is not followed by food. After a few trials, salivation does not occur following stimulation; then the old, well-conditioned points are tried out at various intervals after stimulation at *I*. The results obtained are shown in the accompanying table.

For example, a stimulus is given at *I*: After waiting thirty seconds, a stimulus at any one of the other points causes no secretion. If however, one waits two minutes after giving the initial stimulus at *I* before trying out either *A, B, C* or *D*, a very different result is secured; all of them now cause secretion of saliva, but the quantitative difference is obviously relative to the distance of each of these points from *I*.

These are not isolated experiments, but are easily repeated and follow closely the given quantitative values. They indicate that this differential inhibition spreads rapidly and inhibits stimuli from the four

other points, then gradually retracts until only "*I*" gives inhibition. Pawlow considers that this means that a point on the cortex corresponding to "*I*" (*I'* in fig. 8) is inhibited, and that this process spreads over the cortex to other points, *D'*, *C'*, *B'* and finally, *A'* (corresponding to *D*, *C*, *B* and *A* in fig. 7); then when stimulation of "*I*" is stopped, the process recedes and again "concentrates" at *I'*.

Some dogs are especially sensitive to inhibitory stimuli, the spread of which is believed to advance rapidly over the whole cortex, causing sleep. Some of the experimental animals can be put to sleep in a few seconds by repeated inhibitory stimulation. All these observations are of great importance physiologically and psychologically. Adie [19] recently has even made use of them to explain pathologic conditions. One must still reserve judgment, however, concerning the cortical localization; the theoretical explanation is still too diagrammatic, and the experimental evidence for this phase of the subject is lacking.

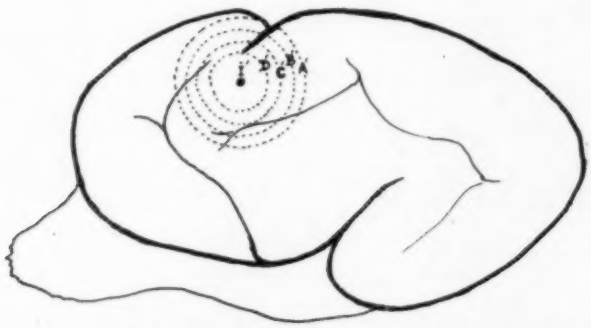

Fig. 8.—Dog's brain showing theoretical spread of inhibition from a point *I'* on the cortex. This figure should be compared with figure 7.

Even the general mechanism of internal inhibition is not well understood, but the observed facts would seem to fit in with the "chemical" theory here described (vide supra).

INHIBITION IN THE PSYCHIATRIC SENSE

What a step it is from these demonstrable forms of inhibition (unknown though their mechanisms may still be) to the inhibitions of the psychologist and the psychiatrist! McDougall's [20] "drainage theory," in which "the activity of each system brings about as a collateral effect the inhibition of all others," is a process allied to "attention"—quite a different thing from Pawlow's conception. Psychiatrists often use the term in a broad way; they may partly apply Pawlow's idea of a balanced

19. Adie, W. J.: Brain **49**:284, 1926.

20. McDougall, W.: Physiological Psychology, London, 1905.

action—a sort of perpetual conflict between excitation and inhibition—but they frequently make the process general, affective and nonspecific. For example, Hart [21] said:

"Suppose that a complex is for some reason out of harmony with the mind as a whole, perhaps because of its intrinsically painful nature, perhaps because it prompts to actions which are incompatible with the individual's general views and principles. In such a case a state of "conflict" arises, a struggle, as it were, between the complex and the personality. These two forces will tend mutually to inhibit each other, the mind will be divided against itself, and a paralysis of action will ensue."

Another quotation that gives a good contrast with the present physiologic conception of inhibition is found in William James' [22] discussion of "will." He said:

"There is a normal type of character, for example, in which impulses seem to discharge so promptly into movements that inhibitions get no time to arise. These are the "daredevil" and "mercurial" temperaments, overflowing with animation and fizzling with talk, which are so common in the Slavic and Celtic races, and with which the cold-blooded and long-headed English character forms so marked a contrast. Simian these people seem to us, whilst we seem to them reptilian. It is quite impossible to judge, as between an obstructed and an explosive individual, which has the greater sum of vital energy."

Here one is in the midst of a discussion of personality, with all its background of racial, endocrine and cultural inheritances and great variations of environment; yet the "inhibitions," or the lack of them, is held to be the greatest factor. Again, on another page of the same book, James said:

"In other persons of the neurotic class there is such a native feebleness in the mental machinery that before the inhibitory ideas can arise the impulsive ones have already discharged into act."

"Inhibitory ideas!" Quite a difficult concept if one tries in any way to hold to the simpler physiologic explanations. It is certainly better in these personality and ethical problems to use another word. What these persons appear to lack is not an inhibitory mechanism, but sufficient associative mechanism to abstract knowledge from past experience and to form a concept which clearly forecasts for them the logical and probable results of their acts. One might say that they lack a sufficient amount of cerebral cortex. Without cerebral cortex, a mammal is an obviously reflex mechanism. With a relatively small amount of cortex, carnivora show good memory (fig. 9) but little power of abstraction. It is only in the higher apes that abstract thought processes become conspicuous.

21. Hart, Bernard: The Psychology of Insanity, Cambridge, 1912, p. 78.

22. James, William: Psychology, New York, 1907, p. 437.

A chimpanzee can sit still and "put two and two together"; he can think out a simple problem and then act on his conclusion.[23] Here one begins to recognize the machinery that James speaks of as "inhibition." It is an increasing possibility of association; an elaboration of what Fulton [24] calls "long-circuiting." In man alone does this mechanism reach high development. In a man with average intellect and experience, any adequate stimulus will set up such a train of association that many memories are aroused and many experiences brought to mind, even experiences of others. Possible acts and their possible results "come to mind." In fact, this very process *is* mind. Those with it do not act "impulsively" on the basis of reflex short-circuits; they act but with delayed, complicated and highly integrated reflexes. They use an extensive "long-circuiting" mechanism and hence act "thoughtfully."

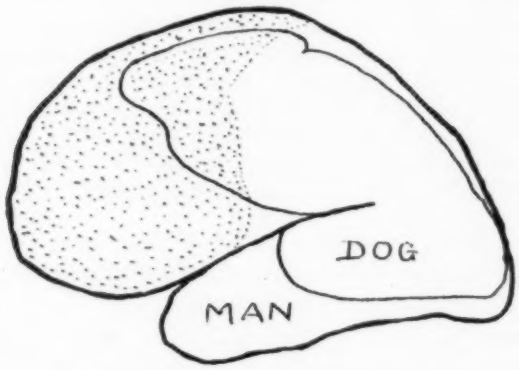

Fig. 9.—Diagrammatic comparison of left cerebral hemispheres of man and dog. An outline sketch of the dog's hemisphere is superimposed on that of a child. The dotted area indicates the frontal lobe of each.

There is no evidence that James' inferior persons have less cortical substance than scholars and captains of industry. But there is evidence that they use less associative power; that in meeting a problem they bring fewer memories to bear and, especially, that they do not look ahead—the power of abstraction is in abeyance. Without a highly developed cerebral cortex, abstraction is impossible; possessed of this organ one may or may not make use of it. The reasons why it is or is not made use of are individual and immensely complex. The point I wish to make here is that the employment of a rich associative mechanism delays immediate reflex action, and allows the motor result of a stimulus —the behavior—to be a considered and thoughtful act, the resultant of the total personality.

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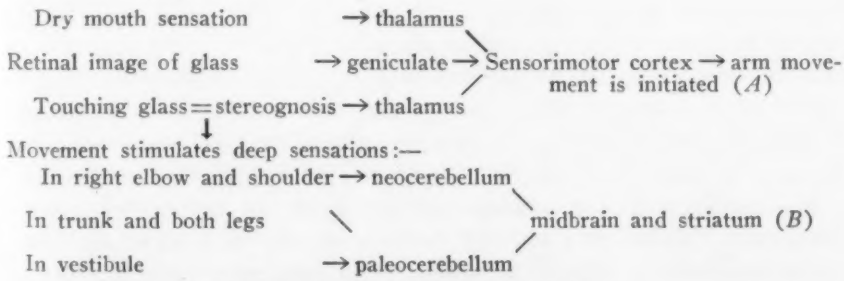
23. Kohler, W.: Psychologies of 1925, Worcester, Mass., 1926.

24. Fulton (footnote 2, second reference).

It may now be seen that "inhibition" is a term used in more than one sense. Before employing it, one should define whether it is the intention to use it physiologically, basing its meaning on some experimental data, or in the vulgar sense, indicating suppressive mental processes, such as "the inhibitions of a New England conscience." There is still as wide a gap between these two uses of the word as there is in my diagram (fig. 1) between physiology and dynamic psychology. Years of work will surely close this gap, but when the knowable is known and the physiologic mechanism of inhibition in the central nervous system is understood, I doubt if it is found to have anything to do with the associative processes here mentioned, which cause thoughtful, deliberate behavior. In the interests of clear thinking, one ought to begin now to define the old terms and to coin new and more specific ones, for physiology will some day surely meet psychiatry, and with forethought a Babel can be avoided.

Throughout most of this lecture, I have used what Fulton [24] called "the permissible analogy" of looking on the central nervous system as a great telephone exchange. In this attitude, the reflex arc is considered as the simple unit—the integer—out of which is built up—integrate—in Sherrington's sense, all complex behavior. So, in analyzing any motor phenomenon, the physiologist visualizes it in terms of reflexes traveling along certain conduction paths, joining others, and cooperating in anatomically understood complexes. For example, I pick up a glass of water: The primary stimulus is a dry mouth, but to accomplish the act many other sensory pathways are brought into play with their appropriate motor discharges. A schema of this simple act is shown in the following:



(*A*) = the "voluntary," "conscious," movements.
(*B*) = the "unconscious" automatic group of postural reflexes.
A supported by *B* carries out the act.

This sort of analysis requires a knowledge of anatomy and of reflex physiology. It is essential in clinical neurology, but it is inadequate for an understanding of the highest levels of integration. At these levels, neurologizing and mechanistic explanations are as yet unsound; they often serve merely as a balm to conscience (by repeating big words borrowed from neurologic nomenclature, one deceives oneself into

believing that one has explained some mental process). This has often been done to explain inhibition. Adolf Meyer aptly names it "neurologizing tautology." Much as such pioneer work as that of Pawlow is to be admired, I think that in his explanations of conditioned reflexes and inhibition he may be neurologizing too much; for instance, when he localizes these processes in the cortex and when he explains in anatomic terms such phenomena as the irradiation and concentration of inhibition.

All this means that one must not be too narrow in one's view of the central nervous system—one must look on it not only as a reflex mechanism, but also as a whole—as a mechanism already built and working. What is its general build? Such contemplation gives a new attitude, analogous to that of the "Gestalt" school of psychologists. Looking on the nervous system as a whole, what then are the important and simple physical properties? Following Kohler's [23] idea, one might enumerate: temperature, pressure, concentration, potential and reaction. These are some of the factors that fundamentally affect Claude Bernard's "internal environment in which the cells live," as opposed to the "external environment in which the organism is placed." One usually is too interested in the latter; neurologists especially are inclined to think of behavior as a complex of reflexes, taking for granted the standard response of the reflex mechanism. Surely the thing is not so simple. A study of the mechanism of respiration [25] shows the remarkable interaction between the two environments: the relative quantities of oxygen and of carbon dioxide in the air affecting the chemistry of the blood, and this, in turn, affecting the nerve centers which control the ventilation, thus regulating the actual amounts of the gases inhaled and exhaled. To explain this mechanism adequately would lead into the intricacies of physical chemistry. For example, it would be necessary to discuss the chemistry of that extraordinary substance hemoglobin, and the physics of the conduction of the nerve impulse. When this point of view of interaction is gained, one feels the inadequacy of the "telephone exchange" concept of the central nervous system. The analogy of a blind impulse running from one center to the next, awakening reflexes as a train of gunpowder sets off a series of blasts, is not satisfactory. All the factors of the internal environment affect all the nerve centers, and all interact to affect the nerve impulses, influencing what they shall be and where they shall go, for the activity of any organized system depends not only on its structure, but on the state of each of its physical variables. In short, allowance must be made for dynamic interaction in the physical sense.

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25. A discussion of these aspects of the question is given in Henderson, L. J.: Certain Aspects of Biochemistry, University of London Press, 1926, and in Gesell: Review, Physiol. Rev. **5**:551, 1925.

FORCED DRAINAGE OF THE CEREBROSPINAL FLUID

IN RELATION TO THE TREATMENT OF INFECTIONS OF THE CENTRAL NERVOUS SYSTEM

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In 1925, the cytology of the cerebrospinal fluid, of the meninges and of the perivascular tissues of the brain was studied in cats that had been subjected to subarachnoid injections of trypan blue (Kubie and Shults [1]). It was found that when the cerebrospinal fluid was drawn from these animals, the proportion of lymphocytes to other cells was consistently higher in the later fractions than in the fluid which drained first. Furthermore, it became clear that after the subarachnoid space was thoroughly drained, the intravenous injection of hypotonic solutions caused the percentage of lymphocytes in the newly formed cerebrospinal fluid to mount even higher. For example, in cat 42 of that study, the lymphocytes of the first drops of fluid constituted 4 per cent of the total number of cells; at the end of the spontaneous drainage, they constituted 80 per cent; after an injection of 0.6 per cent saline solution intravenously, only lymphocytes were found in the final sample of cerebrospinal fluid.

The examination of microscopic preparations from the central nervous system of these cats showed that there had been a remarkable extrusion of the perivascular exudates of lymphocytes into the subarachnoid space. This condition led to the suggestion that such a "washing-out" of inflammatory products from the perivascular tissues might carry with it toxic agents as well, and in this way might influence favorably the course of infections of the central nervous system. It was evident, however, that one would hesitate to employ any therapeutic procedure that might produce a serious increase in intracranial pressure, especially in view of the edema that usually occurs at some stage in infections of the nervous system.

In 1926, therefore, the effort was made (Kubie [2]) to determine how great an increase in intracranial pressure occurs during the administration of hypotonic solutions when the subarachnoid space is drained both before and during the injection of fluid. (The fundamental studies of

1. Kubie, L. S., and Shults, G. M.: Vital and Supravital Studies of the Cells of the Cerebrospinal Fluid and of the Meninges in Cats, Bull. Johns Hopkins Hosp. **37**:91 (Aug.) 1925.

2. Kubie, L. S.: Intracranial Pressure Changes During Forced Drainage of the Central Nervous System, Arch. Neurol. & Psychiat. **16**:319 (Sept.) 1926.

Weed and his collaborators [3] were designed to show the relations between experimental variations of the osmotic pressure of the blood and intracranial pressures under usual physiologic conditions; and for this reason in their work cerebrospinal fluid was not allowed to escape.)

The experiments mentioned (Kubie [2]) were carried out on normal dogs under anesthesia—many of them being subjected to the procedure repeatedly without developing any untoward symptoms (Kubie and Shults [4]). It was found that after draining the canal until the pressure fell nearly to zero the intravenous administration of even 1 liter of from 0.45 to 0.5 per cent saline solution, while allowing the newly formed cerebrospinal fluid to escape through a needle in the cisterna magna, never caused the intracranial pressure to rise to a level above that found in the dogs at the beginning of the experiments. Such a moderate rise in pressure as this, to levels which were usually lower than normal and never above it, could not be looked on as dangerous.

During the past summer, therefore, these studies have been given their first application to human subjects. The object of the present investigation has been to determine: (1) whether thorough drainage of the cerebrospinal fluid in human beings who are suffering from infections of the nervous system is accompanied by an increase in the percentage of lymphocytes in the later fractions of fluid such as occurs in animals; (2) whether the patients tolerate well the administration of hypotonic fluids orally, subcutaneously and intravenously during the later stages of the drainage, and (3) whether the administration of fluids results in a significant increase in the outflow of cerebrospinal fluid, or whether there is any evidence that the free flow of cerebrospinal fluid is blocked under these conditions by swelling of the tissues. It has been possible to answer these questions definitely; but the answer to the ultimate question, whether the procedure has therapeutic value, must await the accumulation of many clinical observations.

The studies have been carried on partly in the Massachusetts General Hospital, through the kindness of Dr. James B. Ayer, and partly in the Children's and Infants' Hospital of Boston, through the kindness of Dr. Bronson Crothers and Dr. Kenneth Blackfan. In addition, seven cases which had been previously studied at the Boston City Hospital by Dr. Perrin H. Long are included in this report. These studies have all been made on patients who were being subjected to lumbar puncture for therapeutic or diagnostic reasons.

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3. Weed, L. H., and Hughson, W.: Intracranial Venous Pressure and Cerebrospinal Fluid Pressure as Affected by Intravenous Injection of Solutions of Various Concentrations, Am. J. Physiol. **58**:101 (Nov.) 1921. Weed, L. H., and McKibben, P. S.: Experimental Alteration of Brain Bulk, ibid. **48**:531 (May) 1919; Pressure Changes in the Cerebrospinal Fluid Following Intravenous Injection of Solutions of Various Concentrations, ibid. **48**:512 (May) 1919.

4. Kubie, L. S., and Shults, G. M.: Studies on the Relationship of the Chemical Constituents of Blood and Cerebrospinal Fluid, J. Exper. Med. **42**:565 (Oct.) 1925.

LYMPHOCYTES OF THE SPINAL FLUID IN MAN DURING THOROUGH DRAINAGE OF THE SUBARACHNOID SPACE

In the accompanying table are presented the total cell counts and the differential counts on first and last fractions of the cerebrospinal fluids of a group of patients, together with the amount of fluid which had been drained between the two fractions. These cases are selected for presentation because the total counts are sufficiently high in most of them so that the percentage changes are significant; it is evident that in the majority, although not in all, the fluid in the last fraction shows a far higher percentage of lymphocytes than that in the first.

Technic.—The samples were collected in tuberculin syringes, and the total counts were made in a Fuchs-Rosenthal counting chamber, after staining with polychrome methylene blue. The differential counts were made both on supravital films (according to the method of Sabin, as described in previous publications, Kubie and Shults [4]) and on drops which had been fixed in iodine vapor, according to the method of Cunningham and Kubie.[5]

The table shows that in drainage of the cerebrospinal fluid the appearance of increasing percentages of lymphocytes in the later fractions of fluid occurs with significant frequency in man as it does in animals. That it does not always take place is not surprising, since the perivascular channels can, at times, be the site of an exudate of polymorphonuclear leukocytes instead of lymphocytes. The occurrence of the phenomenon depends on the existence of a difference in the type of cellular reaction in the depths of the nervous system from that which is taking place at the same moment over the surface. Such differences frequently exist; when this is the case, the cellular response in the perivascular tissues of the depths of the nervous system may reach a lymphocytic stage, while the reaction over the surface is still predominantly polymorphonuclear or phagocytic. Under these conditions, drainage of the subarachnoid space draws off the latter cells first and the lymphocytes later.

In two infants with internal hydrocephalus from meningitis of long duration, it was possible to examine the ventricular and the subarachnoid fluid separately. In both cases the existence of the block was suspected at the time the lumbar puncture was made and was confirmed at autopsy. The subarachnoid fluid from the lumbar region in each case showed the usual increase in lymphocytes in the late fractions, whereas the cellular constitution of the ventricular fluid remained predominantly polymorphonuclear and unchanged throughout. These observations made it possible to eliminate the ventricles as a source of lymphocytes, and

5. Cunningham, R. S., and Kubie, L. S.: Fixation of the Cells of the Cerebrospinal Fluid with Iodine Vapor, Arch. Neurol. & Psychiat. **15**:761 (June) 1926.

The Total Counts and Differential Counts in the First and Last Fractions of Cerebrospinal Fluid

Patient	Hospital*	Hospital Number	Per Cent of Total Cells in First 0.5 Cc. of Cerebrospinal Fluid#			Total Amount of Cerebrospinal Fluid Drained in Cc.	Per Cent of Total Cells in Last 0.5 Cc. of Cerebrospinal Fluid#			Total Cell Count in Cells per C. Mm.		
			Polymorphonuclears	Lymphocytes	Phagocytes		Polymorphonuclears	Lymphocytes	Phagocytes	First 0.5 Cc.	Last 0.5 Cc.	
Acute Anterior Poliomyelitis †												
M. D.	C. H.	108514	60	30	10	25	25	55	15	180	150	
J. D.	C. H.	79776	45	40	15	8	35	60	5	33	54	
J. D.	C. H.	79776	0	75	25	2	0	95	5	16	20	
J. D.	C. H.	108513	20	66	16	40	2	80	18	80	100	
F. M.	C. H.	22903	80	14	6	25	60	30	10	944	896	Soon after intraspinous serum
G. H.	C. H.	107896	95	2	3	10	90	4	6	1,600	800	Soon after intraspinous serum
J. O. S.	C. H.	107896	72	12	16	40	4	78	18	100	360	
J. O. S.	I. H.	?	28	20	52	10	12	52	36	300	450	
J. M.	I. H	?	36	40	30	?	28	60	12	?	?	
J. M.	C. H.	107787	80	16	4	5	52	40	8	83	51	
J. F. N.	C. H.	107800	..	100	..	25	..	100	..	192	102	
I. N.	C. H.	108085	60	40	0	35	20	80	0	24	45	
L. S.	C. H.	108074	20	52	28	30	0	95	5	144	200	
Tuberculous Meningitis												
E. B.	B. C. H.	?	22	78	0	15	4	93	3	210	180	
Septic Meningitis												
E. A.	I. H.	13733	63	15	22	25	51	32	17	2,100	1,700	Meningococcus
J. O'C.	I. H.	13747	98	2	0	5	82	18	0	5,000	5,000	Influenzal
L. G.	B. C. H.	?	76	24	4	18	32	66	2	2,800	3,100	Streptococcus
R. S.	B. C. H.	?	99	1	0	60	28	72	0	4,200	2,200	Pneumococcus
P. C.	M. E. E.	73035	90	6	4	50	94	4	2	2,500	600	Pneumococcus-otogenous
T. H.												
Syphilis of the Central Nervous System												
A. C.	M. G. H.	642922	..	100	..	15	..	100	..	12	4	Early general paralysis ?
N. O.	M. G. H.	284807	12	84	4	25	9	87	4	29	26	General paralysis
N. O.	M. G. H.	284807	5	86	9	40	5	93	2	21	73	General paralysis
E. A.	B. C. H.	?	34	66	0	25	6	93	1	310	200	?
P. J.	B. C. H.	?	40	55	5	22	11	88	1	160	178	?
Serum Reaction in Uncertain Condition												
C. B.	B. C. H.	?	54	42	4	50	18	76	4	2,725	3,200	
C. B.	B. C. H.	?	67	31	2	40	21	72	7	1,200	1,060	
Pertussis and Meningismus												
B. O.	B. C. H.	?	44	52	4	16	12	86	2	98	72	
Combined Sclerosis in Pernicious Anemia (?)												
A. W.	M. G. H.	285002	98	2	0	25	84	14	2	99	68	

* C. H. means Children's Hospital; I. H., the Infants' Hospital; M. G. H., the Massachusetts General Hospital; M. E. E., the Massachusetts Eye and Ear Infirmary, and B. C. H., the Boston City Hospital.

† The high percentage of polymorphonuclear leukocytes is due in many cases to the fact that at varying intervals before the lumbar puncture the patients had received intraspinously human serum from convalescent patients.

In two cases only 25 cells were counted; in two cases only 50; in all of the rest 100 or more cells were seen in each differential count.

demonstrated that the lymphocytes may increase in the subarachnoid fluid as it is allowed to drain from the *surface* of the brain and cord alone without the admixture of any ventricular fluid.

The table also makes it clear that marked changes in the cellular constitution of the cerebrospinal fluid can sometimes occur, even on withdrawal of only small amounts of fluid. In general, it was true that when the pressure was low and the volume of the spontaneous flow was small, the changes in the lymphocyte count were as marked as those which took place when the spontaneous flow of fluid was abundant and under increased pressure. It seems most reasonable to suppose that in the former case the withdrawal of even a small amount of fluid allows the extrusion of the perivascular exudate to begin, whereas in the latter case this can start only after the ventricles and the subarachnoid space have been relieved of a large volume of fluid under high tension.

INITIAL EFFECTS OF ADMINISTRATION OF FLUID

Although no attempt was made to give the procedure a thorough therapeutic trial, fluids were administered to thirteen patients after the cerebrospinal fluid was drained until its spontaneous flow had almost ceased. Hypodermoclyses of from 200 to 300 cc. of from 0.45 to 0.9 per cent saline solutions were given to three infants; intravenous injections of similar volumes of the same solutions were made in five cases; water and diluted orange juice were given by mouth in five more, in amounts of from one to five cupfuls.

In no case did the administration of fluids produce any ill effects, either immediate or delayed; and in no case were there any complaints of subjective discomfort. This is all the more striking because in three of the cases of acute anterior poliomyelitis there were high bulbar symptoms; and in these cases respiratory difficulties from acute increase in intracranial pressure or from local edema might well have been expected.

On the other hand, the fluid administered orally and intravenously produced a striking effect on the outflow of cerebrospinal fluid, while the results of the hypodermoclyses were definite but less marked.

Even small intravenous injections (from 100 to 200 cc.) of a saline solution which contained just enough salt to prevent hemolysis of erythrocytes (from 0.48 to 0.5 per cent) caused a prompt renewal of the outflow of fluid. This began within from 90 to 120 seconds, and continued throughout the injection and for several minutes afterward. The amount of fluid which flowed out under the influence of this injection varied from 15 to 45 cc. in from five to fifteen minutes.

Water administered by mouth produced fluid in as satisfactory a manner after a longer initial lag. The delay is clearly due to the time necessary for passage through the stomach into the intestines. From a child, aged 7, with poliomyelitis, only 3 cc. of cerebrospinal fluid drained

† The high percentage of polymorphonuclears in this case was due to the fact that the patient received intraspinously human serum from convalescent patients.
‡ In two cases only 25 cells were counted; in two cases only 50; in all of the rest 100 or more cells were seen in each differential count.

spontaneously; then all outflow ceased for nearly five minutes. After 1¼ cupfuls of weak orange juice had been sipped through a straw, and after a further latent period of four minutes, 11 cc. of spinal fluid flowed out in less than ten minutes. (In a personal communication, Dr. R. G. Spurling of Louisville, Ky., reports that in adults who are drinking several liters of water daily, as much as 1.5 liters of spinal fluid daily will flow through a continuous lumbar drain.)

RESPONSE TO JUGULAR COMPRESSION

In the experiments on anesthetized dogs already mentioned (Kubie [2]), a wide cannula was screwed into the animal's skull after the dura was opened; the arachnoid, however, was left intact. The cannula was filled with mineral oil, to avoid any osmotic interchange with the fluids of the brain itself, and was then connected with a water manometer. In this way intracranial pressures could be followed with great exactness, even while allowing free drainage of the cerebrospinal fluid. In observations on human patients, however, it is possible to secure only indirect data on intracranial dynamics by measuring the effect of compressing the jugular veins. It was reasoned that, in order to be of serious importance, any swelling and edema of the brain tissue would have to be sufficient in amount to cause at least partial obstruction to the free flow of cerebrospinal fluid, either by occlusion of the aqueduct of Sylvius, or by a narrowing of the minute subarachnoid channels around the spinal cord, or through both. If this occurred, jugular compression could not cause the characteristic and normal increase in the volume outflow of cerebrospinal fluid, nor the normal rise of intracranial pressure as measured with a manometer connected to the lumbar needle. Either volume outflow or manometric response was recorded, therefore, in every case, and it was found that after the administration of fluids the responses to jugular compression were just as prompt and just as extensive as under ordinary conditions. It seems to be a justified conclusion, therefore, that in human beings with infections of the nervous system, just as was found in normal dogs, the amount of diffuse swelling or edema of the brain which this procedure causes is negligible.

COMMENT

In 1924, Dandy [6] reviewed the subject of continuous drainage of the intracranial spaces in cases of septic meningitis, outlining the repeated efforts that have been made in this direction ever since the first reports

6. Dandy, W. E.: The Treatment of Staphylococcus and Streptococcus Meningitis by Continuous Drainage of the Cisterna Magna, Surg. Gynecol. & Obst. **39**:760 (Dec.) 1924.

by Percival Potts in 1760. His review makes it unnecessary to cover the same ground. In general, however, it is possible to draw these conclusions from the clinical and experimental tests which have been reported:

1. All infections of the nervous system and its envelopes are so inconstant in severity and course that conclusions concerning the therapeutic value of any procedure may be safely drawn only from an experience with many cases and in the hands of many impartial observers.

2. Intermittent drainage alone is of doubtful value.

3. Continuous drainage alone yields occasional successes.

4. The rational point of drainage is through the subarachnoid space and not through the ventricles, except—perhaps—when there is an obstructive hydrocephalus.

5. Irrigation of the surface with antiseptic solutions or saline solution is probably not only valueless but actually harmful in many instances, the outstanding exception, perhaps, being when specific serums are available.

Dandy reported three cases of streptococcus meningitis, in two of which the patients recovered, and one case of staphylococcus meningitis with recovery. In the protocols of the three recoveries, it is recorded that fluids were forced, apparently as part of the routine treatment in infectious diseases; but the fact that only from 100 to 300 cc. of cerebrospinal fluid was obtained daily indicates either that the fluids were not forced vigorously, or else that the drainage was not unobstructed. Dandy drained at the cisterna magna, and while this would seem to have certain advantages, these are outweighed by the fact that in no posture can this region be made the lowest point for the whole extent of the nervous system: with the feet high, the head will drain poorly, and with the head high the cord will drain poorly.

Dr. R. G. Spurling (personal communication) from the Louisville City Hospital, Louisville, Ky., is reporting the results of successful treatment in cases of septic meningitis, and in at least one case of poliomyelitis, by continuous lumbar drainage and forcing of fluids to the extent of from 5 to 7 liters daily. In this way, he has obtained over 1 liter of cerebrospinal fluid each day for long periods. Under these conditions, the function of the kidney seems to be supplanted to some extent by the elaboration of fluid through intracranial vessels, confirming the observations made on dogs in the experiments already mentioned (Kubie [2]). Somewhat similar successful efforts with this procedure have been made by Penfield [7] at the Presbyterian Hospital, New York.

7. Reported before the Pediatric Section of the New York Academy of Medicine, April, 1927.

The results of both of these therapeutic tests must be awaited with interest. It is not in purulent forms of meningitis alone, however, that one might hope for favorable results from forced drainage; this may perhaps eventuate in any infection of the nervous system in which the occurrence of perivascular inflammatory reactions suggests that the disease process has some tendency either to invade or else to localize along these pathways, such as poliomyelitis, epidemic encephalitis, syphilis and other conditions.

Throughout this study, the point of view has been implicit that the transudation of fluid from the intracranial blood vessels occurs not only through the choroid plexus, but also through the vessels of the parenchyma, provided that during the administration of fluids there is an opportunity for the simultaneous escape of the newly formed cerebrospinal fluid. This point of view was first suggested by the direct observations on the extrusion of lymphocytes from the perivascular channels into the subarachnoid space. It receives additional support from the demonstration here that the ventricles do not play any part in the production of this phenomenon. Still further evidence of other kinds has accumulated to support the theory and will be presented at another time.

The extent to which this procedure of "forced drainage" can increase the transudation of chemotherapeutic agents and of immune bodies from the blood stream into the nervous system remains to be investigated.

SUMMARY

1. In a wide variety of infections of the nervous system in man, the different types of cells are not homogeneously distributed throughout all fractions of the cerebrospinal fluid; in a significant number of cases the last fractions that appear on lumbar puncture contain a much higher percentage of lymphocytes than the fluid which drains out first.

2. In two cases of meningitis in which the inflammatory reaction had occluded the foramina of Magendie and Luschka, the lumbar spinal fluid still showed this significant increase in lymphocytes in its later fractions, whereas the ventricular fluid contained only polymorphonuclear leukocytes and macrophages throughout all its portions.

3. Marked increases in the percentage of lymphocytes may be observed, at times, even after only small amounts of fluid have been withdrawn. In such cases, the amount of spontaneous flow is usually small, and the initial pressure is usually low.

4. The administration of hypotonic fluids orally, subcutaneously or intravenously during lumbar puncture causes an abundant additional flow of cerebrospinal fluid without subjective distress, respiratory difficulties or evidence of diffuse swelling of the brain tissues.

5. Evidence is accumulating that, under these conditions, the formation of cerebrospinal fluid occurs not only through the choroid plexus, but also by transudation through all of the vessels of the parenchyma.

CONCLUSIONS

It has been shown that it is both safe and rational to combine a maximal forcing of fluids with the principle of continuous or frequent drainage of the cerebrospinal fluid in the treatment of patients with infectious diseases of the central nervous system. The ultimate utility of this procedure, and its limitations, can be established only by clinical experience.

STUDIES IN STUTTERING

III. A STUDY OF CERTAIN REFLEXES DURING STUTTERING *

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AND
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The two previous studies of this series [1] indicated that clonic and tonic spasms, a marked degree of tension and increased activity of certain of the muscles of speech occur during stuttering; in addition to the muscles of speech, it is to be observed commonly that during the act of stuttering other and irrelevant muscles may also be involved: blinking of the eyes, grimacing of the face, tensing of the arms, hands, legs, back or neck, snapping the fingers, stamping the feet, etc. From these observations one obtains the impression that a considerable amount of energy is being liberated as a sort of motor overflow into unrelated channels during the attempt to speak; this study was undertaken to determine whether there might be an observable effect of such overflow on certain reflexes.

Work bearing directly on this point has not been found. Lombard [2] (1887), Dodge [3] (1911) and Tuttle [4] (1925) found that any condition which raises tone in one muscle group tends to raise it in others, and Tuttle (1924) also showed that mental activity such as that involved in the solution of a problem in arithmetic without vocalization increases the extent of the knee jerk.

For the present study the patellar and achilles reflexes were chosen; they were observed during periods of silence and of speech for the

* This work was carried out in the laboratories of the Iowa State Psychopathic Hospital as part of the program of research in physiology of the brain which is being supported by a grant from the Rockefeller Foundation. Additional funds have also been supplied by the Graduate College of the State University of Iowa.

1. Travis, L. E.: Studies in Stuttering: I. Dysintegration of the Breathing Movements During Stuttering, Arch. Neurol. & Psychiat. **18**:673 (Nov.) 1927; II. Photographic Studies of the Voice in Stuttering, ibid. **18**:988 (Dec.) 1927.

2. Lombard, W. P.: The Variations of the Normal Knee-Jerk in their Relation to the Activity of the Central Nervous System, Am. J. Psychol. **1**:5 (Nov.) 1887.

3. Dodge, R.: A Systematic Exploration of a Normal Knee-Jerk, Ztschr. f. allg. Physiol. **12**:1, 1910.

4. Tuttle, W. W.: The Distribution of Tone in Skeletal Muscle, J. Exper. Psychol. **8**:319 (Aug.) 1925; The Effect of Attention on Mental Activity on the Patellar Tendon Reflex, ibid. **7**:401 (Dec.) 1924.

normal speaker, and periods of silence, stuttering and free speech for the stutterer. Free speech of the stutterer is used to designate periods when he is able to produce a flow of speech without observable signs of stuttering.

APPARATUS AND METHOD

The two types of apparatus for eliciting these two reflexes have been described by Tuttle and one of us (Travis [5]) in 1927. Briefly, they automatically deliver blows of uniform intensity at a constant rate to the patellar or achilles tendon.

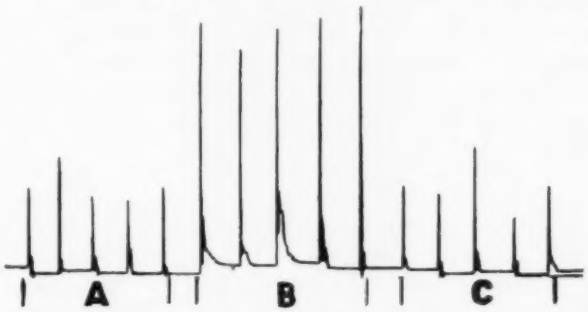

Fig. 1.—Record of knee jerk of a normal speaker. *A* indicates silence before speech; *B*, during free speech, and *C*, silence after speech. In this figure and in figures 2, 3 and 4, there is a difference in extent between the responses of the normal and those of the stutterer in the records of each reflex chosen for illustration. This, however, is a chance factor of individual variation, and other records show that this difference is not characteristic.

Fig. 2.—Record of knee jerk of a stutterer. *A* indicates silence before speech; *B*, *D* and *G*, during stuttering, and *C*, *E* and *F*, during free speech.

In this experiment, however, the blows were not delivered at a constant rate but at the proper time to accord with the speaking and stuttering periods which were too uncertain in order of appearance and duration to permit the jerks to be elicited at a constant rate. The recording was done on a smoked drum. In the case of the knee jerk a reducing pulley was placed between the leg and the stylus, so that the record shows only one unit for 3.75 units of the actual jerk. In the tables, however, the original measurements have been multiplied by 3.75 so that the figures represent the actual extent of the jerks. A reducing pulley was

5. Tuttle, W. W., and Travis, L. E.: A Comparative Study of the Extent of the Knee-Jerk and the Achilles-Jerk, Am. J. Physiol. **82**:147 (Sept.) 1927.

not used for the achilles reflex. Our records present a certain apparent qualitative difference in the knee jerks of the stutterer and those of the normal speaker. It consists of a clonic movement of diminishing range which follows the initial reflex contraction and which is much more striking in the record of the stutterer (fig. 2) than in that of the normal speaker (fig. 1). We have not attempted as yet a study of qualitative differences. For our present purpose, comparisons are not to be made between different persons but merely between different periods for the same person.

For the knee reflex the subjects were seated, while for the achilles reflex they were placed in a prone position. In both instances, leg supports kept the point of application of the stimuli in a fixed position.

As a contemporary study, an investigation was made into the resistance to passive displacement of the relaxed hand. This was carried out by means of an apparatus described by Travis [6] (1924), which measures the resistance in arbitrary units that the pendant hand offers to an unexpected blow.[7] The blows were of uniform intensity and of approximately 40 ounces. They were delivered to the

Fig. 3.—Record of achilles reflex of a normal speaker. *A*, *B* and *C* represent the same periods as in figure 1.

Fig. 4.—Record of achilles reflex of a stutterer. *A* indicates silence before speech; *B*, *C*, *D*, *F*, *H*, *J* and *M*, during stuttering, and *E*, *G*, *I*, *K* and *L*, during free speech.

back of the hand. The forearm, held in a sling, was permitted by the subject to assume a position determined by gravity. Under no circumstances was the subject himself to make any attempt to determine or change the position of the arm or hand during an experiment. Extreme care was taken to keep the entire body of the subject in a fixed position.

Factors entering into the resistance offered by the hand to an unexpected blow are not entirely clear, but the inertia of the pendant hand and the tonus of the extensor muscles of the wrist are two obvious ones. As the inertia is a constant for the same person during an experiment, the changes in resistance offered by the hand for the various periods studied are probably referable to changes in muscular tonus of the extensor muscles of the wrist.

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6. Travis, R. C.: A Study in the Measurement of Muscle Tonus and Its Relation to Fatigue, J. Exper. Psychol. **7**:201 (June) 1924.

7. The blows were unexpected in the sense that the subject did not know the exact time they would fall.

In both studies, that of the reflexes and that of the resistance offered by the pendant hand, the subject was handled in the same manner. Before any records were made, he was permitted to take his position and remain quiet for ten minutes. After this interval of quiet, the experiment was begun.

For the normal speaking person, an experiment consisted of three parts: the silent period before speech, the period of speech and the silent period after speech. For the stutterer the periods of stuttering and free speech do not follow any definite order but, naturally, must be studied as they occur. Our readings are tabulated, however, as the silent period before speech, the period of stuttering,

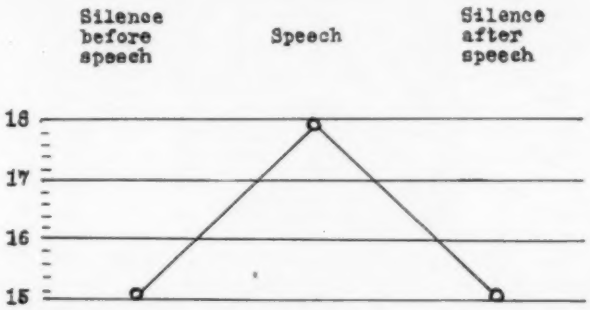

Fig. 5.—Graph of resistance readings from a normal speaker. The vertical column of figures in this figure and in figure 6 indicate the amount of resistance offered by the pendant hand in terms of an arbitrary scale.

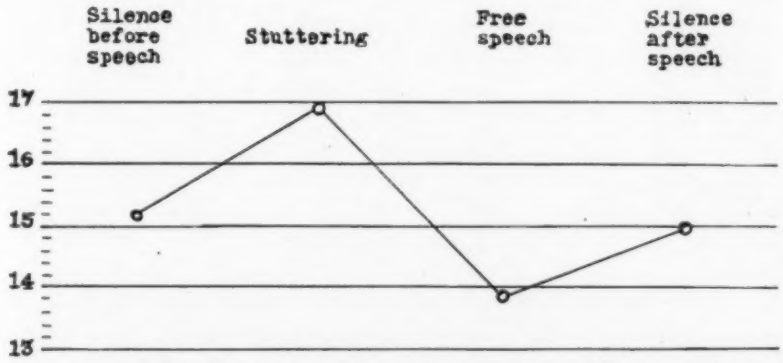

Fig. 6.—Graph of resistance readings for a stutterer.

the period of free speech and the silent period after speech. For both normal speakers and stutterers the speech studied was of the unemotional propositional type concerning current topics of interest.

During the elicitation of the reflexes and the striking of the hand for all periods the subject kept his eyes closed, and as all of the apparatus used in the studies was practically noiseless the subject could not anticipate the application of the stimulus.

All except three of the subjects were students in the university between the ages of 18 and 30 years. The three exceptions were stutterers in the elementary schools varying in ages from 12 to 15 years.

DATA

Tables 1 to 6 present the data from a study of the two reflexes of the right leg and from a study of the resistance offered by the pendant hand of the right side on nine stutterers and ten controls.

TABLE 1.—*Data of the Reflexes of the Right Leg of Normal Speakers*

	Knee Reflex			Achilles Reflex		
Case	Silence before Speech	During Free Speech	Silence after Speech	Silence before Speech	During Free Speech	Silence after Speech
A	26.2*	57.2	22.0	9.4	24.2	6.0
B	25.2	45.0	28.4	10.1	11.5	10.5
C	26.2	44.2	19.1	11.0	12.2	10.0
D	32.0	37.5	27.8	6.2	7.6	4.8
E	12.4	30.1	19.9	2.1	3.1	2.0
F	22.0	34.3	23.0	7.0	9.0	7.0
G	38.7	101.2	52.9	3.1	4.1	3.0
H	29.4	63.7	21.4	6.4	10.0	2.8
I	43.5	115.8	62.6	5.2	7.4	5.6
J	44.2	103.1	54.0	3.4	5.5	3.6
Mean	30.0	63.2	33.1	6.4	9.5	5.5

* All figures in this table and tables 2, 7 and 8 indicate the extent of reflexes in millimeters. Each figure in tables 1, 2, 4, 5, 7, 8, 10 and 11 is the average of at least five readings.

TABLE 2.—*Data of the Reflexes of the Right Leg of Stutterers*

	Knee Reflex				Achilles Reflex			
Case	Silence before Speech	During Stuttering	During Free Speech	Silence after Speech	Silence before Speech	During Stuttering	During Free Speech	Silence after Speech
A	28.8	30.0	23.2	28.0	2.6	4.9	2.4	2.8
B	19.4	25.0	21.3	20.0	5.6	8.3	5.6	5.6
C	42.4	68.0	41.2	41.2	14.5	16.0	12.5	13.5
D	24.2	37.0	22.0	25.0	5.1	7.0	4.9	6.0
E	78.7	101.2	58.9	82.5	12.0	13.4	11.0	11.8
F	50.2	61.1	34.5	51.7	7.2	7.2	5.5	7.3
G	75.0	100.1	45.7	48.7		No reflex elicited		
H	33.7	60.0	27.3	30.0		No reflex elicited		
I	41.2	103.1	34.5	41.0	11.1	16.6	9.7	10.8
Mean	39.4	58.5	30.9	36.8	8.3	10.5	7.3	8.3

TABLE 3.—*Summary of Data of Reflexes of the Right Leg*

	Knee Reflex				Achilles Reflex			
Group	Silence before Speech	During Stuttering	During Free Speech	Silence after Speech	Silence before Speech	During Stuttering	During Free Speech	Silence after Speech
Normal speakers	30.0	....	63.2	33.1	6.4	....	9.5	5.5
Stutterers	39.4	58.5	30.9	36.8	8.3	10.5	7.3	8.3

Tables 1 and 2 show: (1) that every normal speaker studied gave an increase in the extent of both reflexes during speech; (2) that every stutterer studied gave an increase in extent of both reflexes during stuttering, except in one in whom the achilles reflex during stuttering was equal to but did not exceed that during silence before speech;

(3) that every stutterer studied gave a decrease in extent of both reflexes during free speech except two. In one of these the achilles reflex during the period of free speech was equal to that obtained during the silent periods. In the other the knee jerk during free speech was equal to that obtained during the silent period after speech, although

TABLE 4.—*Data from the Right Pendant Hand of Normal Speakers*

Case	Silence before Speech	During Free Speech	Silence after Speech
A*	14.8	17.7	15.0
B*	20.4	21.7	20.6
C*	17.2	18.5	17.2
D	17.2	18.2	17.0
E	12.2	13.8	12.9
F	16.5	17.6	15.9
G	20.4	21.7	20.6
H	18.8	20.0	18.8
I	14.8	17.7	15.0
J	11.4	15.3	11.4
Mean	16.4	18.2	16.4

* The first three subjects in this table and the first three stutterers in table 5, the latter being three of our most severe cases, were tested daily for twelve days. Each of these six persons furnished at least sixty readings for each period, and the entries following their names are the averages of sixty or more readings.

TABLE 5.—*Data from the Right Pendant Hand of Stutterers*

Case	Silence before Speech	During Stuttering	During Free Speech	Silence after Speech
A	15.9	17.0	14.4	16.6
B	16.7	18.4	15.1	16.4
C	20.1	21.5	18.8	19.8
D	13.5	14.3	12.7	13.2
E	15.1	17.3	13.4	14.8
F	15.7	16.9	15.0	15.4
G	17.2	18.1	14.8	17.5
H	15.1	16.9	13.9	15.1
I	10.7	11.1	9.7	9.8
J	15.9	16.9	14.9	16.1
Mean	15.6	16.8	14.4	15.5

TABLE 6.—*Summary of Data from the Right Pendant Hand*

Group	Silence before Speech	During Stuttering	During Free Speech	Silence after Speech
Normal speakers	16.4	....	18.2	16.4
Stutterers	15.6	16.8	14.4	15.5

less than that during the silent period before speech. None of these exceptions are considered important.

Tables 4 and 5 show: (1) that every normal speaker studied offered more resistance to an unexpected blow to the hand during speech; (2) that every stutterer studied offered more resistance to an unexpected blow to the hand during stuttering; (3) that every stutterer offered less resistance to an unexpected blow during free speech.

TABLE 7.—*Data of Reflexes of the Left Leg of Normal Speakers*

	Knee Reflex			Achilles Reflex		
Case	Silence before Speech	During Free Speech	Silence after Speech	Silence before Speech	During Free Speech	Silence after Speech
A	49.5	177.4	69.0	7.0	23.0	7.0
B	29.4	32.0	31.0	5.7	6.3	6.0
C	68.6	153.7	97.5	15.0	17.6	16.5
Mean	49.1	121.0	65.8	9.2	15.6	9.8

TABLE 8.—*Data of Reflexes of the Left Leg of Stutterers*

	Knee Reflex				Achilles Reflex			
Case	Silence before Speech	During Stuttering	During Free Speech	Silence after Speech	Silence before Speech	During Stuttering	During Free Speech	Silence after Speech
A	35.6	45.0	18.7	30.7	12.2	15.5	10.0	13.0
B	48.7	62.0	37.5	47.9	12.0	14.0	12.0	11.0
C	93.7	118.5	75.4	61.5	15.0	17.2	14.6	14.3
Mean	59.3	75.1	43.8	46.7	13.1	15.6	12.2	12.8

TABLE 9.—*Summary of Data of Reflexes of Left Leg*

	Knee Reflex				Achilles Reflex			
Group	Silence before Speech	During Stuttering	During Free Speech	Silence after Speech	Silence before Speech	During Stuttering	During Free Speech	Silence after Speech
Normal speakers	49.1	....	121.0	65.8	9.2	....	15.6	9.8
Stutterers	59.3	75.1	43.8	46.7	13.1	15.6	12.2	12.8

TABLE 10.—*Data from Left Pendant Hand of Normal Speakers*

Case	Silence before Speech	During Free Speech	Silence after Speech
A	15.6	18.6	16.0
B	10.8	12.4	12.0
C	9.7	10.8	9.5
D	10.9	12.8	11.1
E	10.2	10.6	9.3
Mean	11.4	13.0	11.6

TABLE 11.—*Data from the Left Pendant Hand of Stutterers*

Case	Silence before Speech	During Stuttering	During Free Speech	Silence after Speech
A	11.2	10.7	10.0	10.8
B	12.2	12.4	10.7	11.5
C	10.8	11.2	10.7	11.2
Mean	11.4	11.4	10.4	11.1

TABLE 12.—*Summary of Data from the Left Pendant Hand*

Group	Silence before Speech	During Stuttering	During Free Speech	Silence after Speech
Normal speakers	11.4	....	13.0	11.6
Stutterers	11.4	11.4	10.4	11.1

Tables 7 to 12 give the data from a study of the two reflexes of the left leg and from a study of the resistance offered by the pendant hand of the left side to an unexpected blow. This part of the study has as yet been carried out on only a small number of patients. The results obtained on the left leg and hand from this limited experiment are, however, comparable to those obtained from the right leg and hand.

SUMMARY

The knee and achilles reflexes in our cases have shown a greater amplitude during the speech of normal speakers than during silent periods. In our stutterers there is, during stuttering, an increase in amplitude comparable to the speaking period of the normal speakers. When the stutterer is speaking freely, in contrast to the period of free speech of normal speakers, the amplitude of the two reflexes is less than during silent periods. The variations in resistance offered by the pendant hand to an unexpected blow are directly comparable to these reflex variations.

CEREBELLAR PHENOMENA IN LESIONS OF THE TEMPORAL LOBE *

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The temporal lobe is generally designated as a "silent area" owing to the fact that, aside from the superior temporal convolution which, as indicated by the experimental studies of Ferrier [1] and Munk,[2] is the center for the perception of auditory stimuli, this part of the brain is not known to represent a specific function. Extirpation of this part in animals (the dog and the ape) is found to produce, not a loss of motor or sensory functions, but only a state of mental torpor, the animal losing its initiative and becoming tardy in reactions to environmental stimuli. In other words, the animal apparently does not lose an individualized function, but only fails to perform properly the functions presided over by other parts of the brain.

Clinicopathologic studies in man have likewise localized a specific function in the superior convolution of the lobe only. This convolution, in addition to subserving the function of hearing, as taught by von Monakow,[3] Bruns [4] and others, is believed to be the focal region, on the left side in right-handed people and on the right side in left-handed people, for the comprehension of auditory speech. The rest of the lobe is not known to represent a specific function, either motor or sensory, and, as taught by Flechsig on the basis of myelinization studies, is assumed to be an association center for the integration of the physiologic activities of other centers in the brain. Motor and sensory symptoms have been known to occur in lesions of the temporal lobe and have generally been explained by the supposition that the lesion involved, either directly or by pressure, the neighboring motor or sensory structures. Included with these symptoms were also the cerebellar phenomena so frequently noted in tumor or abscesses of this lobe; these were regarded as the result of pressure from a distance on the cerebellum.

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* Read before the Section on Nervous and Mental Diseases at the Seventy-Eighth Annual Session of the American Medical Association, held at Washington, D. C., May 20, 1927.

1. Ferrier, D.: Functions of the Brain, Smith, Elder & Company, 1876, chapter 9.

2. Munk, H.: Ueber die Funktionen der Grosshirnrinde, ed. 2, Berlin, A. Hirschwald, 1890, p. 12.

3. Von Monakow: Gehirnpathologie, ed. 2, Vienna, Alfred Holder, 1905, p. 312 and 930.

4. Bruns, L.: Die Geschwülste der Nervensystems, Berlin, S. Karger, 1908, p. 146.

CEREBELLAR PHENOMENA IN LESIONS OF THE TEMPORAL LOBE

Cerebellar phenomena in lesions (tumors or abscesses) of the temporal lobe have been described by Schupfer,[5] Ulrich,[6] Bruns,[7] Oppenheim,[8] Potts,[9] Mingazzini,[10] Chotzen [11] and others, and more recently by Knapp.[12] The phenomena described by these authors referred to disturbances of function comprised under the general designation of cerebellar ataxia or incoordination.

It was noted that patients with lesions of the temporal lobe are unable to stand on a narrow base without swaying or falling; that in walking they stagger, tending to fall backward, forward or to the sides; that in executing movements with the arms, the limbs make irregular excursions, frequently failing to reach their destination. In describing these symptoms, these authors did not emphasize as a distinct phenomenon uniform falling or uniform past-pointing, phenomena which, while also present in lesions of the cerebellum, originate in a disturbance of function of a different character from that responsible for so-called cerebellar ataxia.

Cerebellar ataxia or incoordination is, in accordance with available clinical and experimental evidence, produced by faulty contraction of the acting muscles. It is brought about, according to Luciani [13] (who maintains that the fundamental phenomena of cerebellar deficiency are asthenia, atonia and astasia), by the contraction of muscles that are deficient in strength, that lack tonus and have lost the capacity for a uniformly maintained contraction. According to Babinski [14] and others,

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5. Schupfer, F.: Gliosarcom in rechten Schlaffenlappen, Monatschr. f. Psychiat. u. Neurol. **24**:63 (July) 1908.

6. Ulrich: Ueber einen Tumor im rechten Temporalhirn, Deutsche Ztschr. f. Nervenh. **40**:1, 1910.

7. Bruns, L.: Neurol. Centralbl., June, 1909, no. 12, p. 665.

8. Oppenheim, H.: Lehrbuch der Nervenkrankheiten, ed. 6, Berlin, S. Karger, 1913, p. 1172.

9. Potts: Tumors of the Right Temporo-Sphenoidal Lobe, J. Nerv. & Ment. Dis. **41**:249 (April) 1914.

10. Mingazzini: Neue klinische und anatomo-pathologische Studien über Hirngeschwülste und Abszesse, Arch. f. Psychiat. Berlin **47**:1028, 1910.

11. Chotzen: Tumor (Gliom) des rechten Schlaffenlappen, Berl. klin. Wchnschr. **1**:736 (April) 1911.

12. Knapp, A.: Pseudo-Zerebellare Schlaffenlappen Ataxie, Deutsche med. Wchnschr. **441**:717 (June, 27) 1918; Die Tumoren des Schlaffenlappen, Ztschr. f. d. ges. Neurol. u. Psychiat. **42**:226, 1918.

13. Luciani, L.: Human Physiology, New York, The Macmillan Company, 1915, vol. 3.

14. Babinski, J.: De l'asynergie cérébelleuse, Soc. de neurol., Nov. 9, 1899.

who claim that the function of the cerebellum is to synergize unit movements into a complex whole, the ataxia is the result of failure on the part of each contracting muscle to act in concord with the others in carrying out a complicated act. According to my theory, based on experimental evidence as reported in previous communications,[15] the ataxia is a manifestation of hypermetria, the excessive contraction of the voluntarily acting muscles resulting from uncontrolled, unrestrained activity of the motor cortex of the cerebrum which is brought about by removal of the inhibition normally exerted by the cerebellum. An entirely different mechanism is at work in the production of uniform falling and uniform past-pointing.

These phenomena, judging from their invariable occurrence on stimulation or following the extirpation of the labyrinth, are produced, not by the faulty contraction of abnormally functioning muscles, but by an effort on the part of the patient to act in accordance with an erroneous conception which he forms, as a result of abnormal stimuli flowing into his consciousness from the vestibular system, concerning his relationship in space and his surroundings. For example, he falls to the right because the information he receives from the labyrinth leads him to think that he is falling to the left; to obviate this, he continually bends over to the right; he past-points to the right because, experiencing as he does the sensation of moving away from the examiner's finger, in order to touch it, he must project his own finger further to the right, etc. Uniform falling and uniform past-pointing have been observed with marked regularity in the cases of tumor of the temporal lobe reported in this communication.

Another cerebellar phenomenon which I observed in tumors of the temporal lobe, not mentioned as such by previous writers on this subject, is diminution or loss of tendon reflexes on the side opposite to the lesion or on both sides of the body. This phenomenon is described also by Knapp,[12] who attributed it to traction by the tumor on the dorsal roots of the cord; this hypothesis appears to me unsatisfactory since the symptoms are not observed in tumors of other regions of the brain. The deep reflexes are directly dependent on the state of tonus in the stimulated muscles. Lesions of the cerebellum, when limited in character and not associated with a simultaneous involvement of the pyramidal tracts, may give rise to diminution or loss of reflexes on the affected side. This may be caused either by the loss of proprioceptive stimuli from the muscles or joints or by the loss of vestibular stimuli (and labyrinthine tonus of Ewald). Such reflex changes were pointed out by me on the

15. Meyers, I. Leon.: Galvanometric Studies of the Cerebellar Function. J. A. M. A. **65**:1348 (Oct. 16) 1915.

basis of experimental studies,[16] and were described by Gordon Holmes [17] as the result of injuries received during war. The diminution or loss of the reflexes observed in the cases of lesions of the temporal lobe here reported is therefore to be regarded as a cerebellar phenomenon.

Included with the cerebellar phenomena that may occur in lesions of the temporal lobe is a coarse tremor in the limbs on the side opposite the lesion. While tremor may occur in lesions of the basal ganglia in the absence of any involvement of the cerebellum, the symptom, according to Luciani, is one of the fundamental manifestations of cerebellar deficiency. As reported in a previous communication,[15] this tremor was observed also with regularity in animals with experimental lesions of the cerebellum.

REPORT OF CASES

CASE 1.—*A glioma of the anterior and posterior portions of the left second and third temporal convolutions, involving also partly the central part of the occipital lobe. Cerebellar phenomena: ataxia of station and gait—tendency to fall uniformly to the right; loss of tendon reflexes in right arm and leg; coarse tremor in right arm and leg.*

History.—R. J., a man, aged 53, married, a mechanic and formerly an officer in the English army, entered the Los Angeles General Hospital on Dec. 22, 1926. His only previous illness was appendicitis, in 1915. The patient was confused as to the time and as to his surroundings; he did not know when the illness began nor for whom he worked. Most of the history was obtained from a friend. However, he answered some questions correctly and executed simple commands in a slow, questioning and hesitating manner. He had been in good health and worked on a fruit sorting machine, doing work which required much alertness, until three months before admission to the hospital, when he began to suffer from headache and somnolence. The headache was frontal and not severe. This was followed by dizziness and attacks of vomiting. The friend reported that after the onset of the illness, while at a party, the patient had an attack of unconsciousness and had to be taken home; a few weeks later, he had another such attack on a street car.

Neurologic Examination.—Neurologic examinations on Dec. 24 and 25, 1926, revealed: pupils small, right somewhat larger than the left; both reacted to light and in accommodation; no nystagmus; tendency to conjugate deviation of the eyes toward the left—he moved the eyes with difficulty to the right. The right disk was choked, showing an elevation of about 3 diopters; the left showed postneuritic atrophy. Hearing was diminished for both air and bone conduction. The right arm and leg appeared to be weak. There was a coarse tremor in the right hand. The wrist and triceps reflexes were absent on the right side. Patellar and achilles reflexes were markedly diminished on the right but normal on the left side. Abdominal and cremasteric reflexes were present on both sides. There was no Babinski sign on either side. The patient was unable to stand on a narrow base; he tended to fall to the right; in walking, he tended to fall forward

16. Meyers, I. Leon: Cerebellar Localization. An Experimental Study by a New Method, J. A. M. A. **67**:1745 (Dec. 9) 1916.

17. Holmes, Gordon: The Symptoms of Acute Cerebellar Injuries Due to Gunshot Injuries, Brain **40**:461, 1918.

and to the right. There were no objective sensory disturbances. Astereognosis was not present but perseveration was marked—for example, when a key was placed in the hand he named it correctly, but continued to name other objects, placed in the hand afterward, "a key," etc.; perseveration was manifested also in writing. Dysarthria was not present. He was unable to read, although a good deal of vision was retained. (The patient was right-handed.) As tested by a finger, right-sided hemianopia was apparently complete.

The Wassermann reaction of the blood was negative. The spinal fluid was under increased pressure and clear; it contained 120 cells per cubic millimeter, 80 per cent of which were lymphocytes; the Wassermann reaction of the fluid was negative; globulin tests revealed a trace. Roentgenograms of the head did not show abnormalities. Neuro-otologic studies were not made. The patient died on Jan. 6, 1927.

Necropsy.—The necropsy revealed a tumor, which microscopically was a glioma. It occupied the anterior and posterior portions of the second and third temporal convolutions on the left side and involved also partly the central part of the occipital lobe.

CASE 2.—*Glioma of left second, third and fourth temporal convolutions, involving also the hippocampal and uncinate gyri. Cerebellar phenomena: uniform past-pointing to the right (in the absence of ataxia as shown by the finger-to-nose test); loss of tendon reflexes on both sides; tremor of the right hand.*

History.—E. B., a newsboy, aged 17, when admitted to the Los Angeles General Hospital, Feb. 26, 1926, was drowsy, somnolent, uncooperative in the examination and answered questions very slowly. He had been well until about three weeks before he entered the hospital when, on returning home from work, he had had an attack of vomiting. He had had headaches and photophobia since that time and also several attacks of vomiting. When admitted to the hospital his only complaints were weakness and lack of "pep." He had had typhoid fever a number of years before, but no other illness.

Neurologic Examination.—Neurologic examinations were made on March 2, 3 and 24, 1926. The patient was in a recumbent posture; he was unable to sit up owing to pain in the back of the head and neck. He was drowsy and apathetic. The right pupil was wider than the left and did not react to light as did the left. He had slight ptosis of the left eyelid and partial paralysis of the right abducens. As tested by a finger, he appeared to have no hemianopic defects. Both disks were choked with an elevation of about 4 diopters; the vessels were prominent and tortuous, and there were several retinal hemorrhages. There was no nystagmus. There was weakness of the face, arm and leg on the right side. There were no objective sensory disturbances and no astereognosis. The tendon reflexes were absent in the arms and legs. Babinski, Chaddock and Gordon signs were absent. The abdominal and cremasteric reflexes were present on both sides. There was a coarse tremor in the right hand. With the left hand the patient past-pointed persistently about 3 or 4 inches to the right, but he showed no ataxia by the finger-to-nose test in that hand. There was no dysarthria. The patient understood spoken language, but answered questions slowly, and carried out commands such as "close your eyes," etc., only after considerable delay. He was rather confused and did not cooperate in tests for writing and reading. He was right-handed.

Neuro-otologic studies were made by Dr. Linthicum. Douching the right ear with water at 68 F. with the head back produced horizontal nystagmus to the left; it was of good amplitude, but the eyes showed a tendency to conjugate deviation to the right at the end of sixty seconds. With the head upright there

was a rotary nystagmus to the left, which was of fair amplitude. Douching the left ear with water at 68 F. with the head back gave rise to nystagmus to the right, which appeared more quickly than that produced by douching the right ear; with the head upright there was rotary nystagmus to the right, which was of good amplitude. The neuro-otologic studies therefore showed that all semicircular canals on each side were functioning normally.

Roentgenograms of the head were normal. The Wassermann reaction with the blood was negative. Spinal puncture showed the fluid under markedly increased pressure; it was clear and with 7 lymphocytes per cubic millimeter; there was a trace of globulin, no increase in sugar, and the Wassermann reaction was negative. The patient died on April 2, 1926.

Necropsy.—Necropsy revealed a tumor of the left temporal lobe which completely replaced the inferior and middle convolutions and extended partly into the hippocampal gyrus.

CASE 3.—*Deep-seated infiltrating glioma of the temporal lobe on the right side, extending also into the parietal and occipital lobes. Cerebellar phenomena: falling to the right and back, occasionally also to the left; past-pointing persistently to the left; tendon reflexes sluggish on the left, variable on the right side, on one occasion absent, at other slightly increased; tremor of the left hand.*

History.—M. L. W., an American woman, aged 59, married, who was the mother of two living children and who had had no miscarriages, entered the Los Angeles General Hospital on Nov. 30, 1925. She had always been in good health until the spring of 1925, when she began to suffer from dizziness, noises in the head and headaches. This was soon followed by attacks of nausea and vomiting, and by difficulty in standing and walking. These symptoms had been growing worse.

Neurologic Examination.—Neurologic examinations made on Nov. 31, Dec. 12 and 14, 1925, and Jan. 12, 1926, revealed: pupils unequal, the right being the larger; both reacted to light and in accomodation; weakness of the left abducens; no nystagmus; pronounced papilledema on both sides; the disk margins were completely wiped out and the vessels buried; no retinal hemorrhages. Gross vision was preserved. Hyperesthesia was present in the region of the fifth nerve on the right. Anosmia was revealed by the oil of wintergreen test. The visual fields could not be studied owing to inattention of the patient. Hearing seemed to be impaired for bone as well as for air conduction on both sides. There was some weakness of the face on the left side, central in type. The arms and legs showed fair strength. There was no definite ataxia by the finger-to-nose test on either side. The tendon reflexes were sluggish on the left side, and on the right on November 31; they were absent on the right side on December 12 and 14, and increased on January 12. The patient was unable to stand on a narrow base even with the eyes open; she tended to fall to the right and back, occasionally to the left. In walking she tended to fall in the same directions. She past-pointed persistently to the left. There was no cerebellar attitude of the head. The sense of position was impaired in the left hand; there was some astereognosis in that hand, and tremor which was increased by voluntary movement. The patient spoke in a monotone, was slow in responding, and carried out commands after much delay; she was inattentive and appeared confused. There was no impairment of motor speech.

Neuro-otologic studies were made by Drs. Isaac H. Jones and S. Jesberg. Douching the left ear with water at 68 F. with the head back produced horizontal nystagmus to the right after twenty-five seconds, which became oblique in the left eye (probably because of the paralysis of the abducens in that eye). Douching

of that ear was continued for three minutes and twenty-five seconds without producing any constitutional response (pallor, sweating and vomiting). When the head was brought upright, nausea and vomiting set in quickly. The patient was too sick for the tests to be continued.

Roentgenograms of the head were normal. The Wassermann reaction of the blood was negative. The patient died on Feb. 21, 1926.

Necropsy.—Necropsy revealed a large, deep-seated and infiltrating glioma in the region of the right temporal lobe, involving to some extent practically every part of that lobe, and extending also into the right parietal and occipital lobes. There were no gross alterations in any other parts of the cerebrum, in the midbrain, pons or cerebellum.

SUMMARY

The three cases reported showed: (1) symptoms due to general increase in intracranial pressure (headache, vomiting, papilledema, and in case 1 attacks of unconsciousness); (2) symptoms referable to pressure on structures adjacent to the growth—contralateral weakness of the face (presumably from pressure on the suprasylvian motor area of the cerebrum), contralateral hemiparesis (from a similar but more extensive effect in the motor cortex) and partial ocular paralysis which in my cases, as in cases reported by Knapp, occurred chiefly on the side opposite to the lesion; (3) the cerebellar phenomena enumerated; (4) mental retardation—a characteristic slowing down of all thought processes in every case; (5) complete homonymous hemianopia in one case; in the other two the patients could not be satisfactorily examined for this symptom.

None of the patients, as far as could be determined, had had uncinate fits, a symptom described in tumors of this region by Hughlings Jackson,[18] Charles K. Mills [19] and others, nor did they have elaborate hallucinations such as have been described by Foster Kennedy,[20] Cushing [21] and others. In not one of the cases was there at any time spontaneous nystagmus, nor (in the cases studied neuro-otologically) a break in the vestibular reflex as manifested by induced nystagmus.

ORIGIN OF THE CEREBELLAR PHENOMENA IN LESIONS OF THE TEMPORAL LOBE

The origin of the cerebellar symptoms has generally been placed in the cerebellum, the phenomena, according to this view, arising from

18. Jackson, J. Hughlings: Case of Tumour of the Right Temporosphenoidal Lobe Bearing on the Localization of the Sense of Smell and the Interpretation of a Particular Variety of Epilepsy, Brain **12**:346, 1890.

19. Mills, Charles, K.: The Cerebral Centers for Taste and Smell and the Uncinate Group of Fits, J. A. M. A. **51**:879 (Sept. 12) 1908.

20. Kennedy, Foster: The Symptomatology of Temporo-Sphenoidal Tumors, Arch. Int. Med. **8**:317 (Sept.) 1911.

21. Cushing, Harvey: The Field Defects Produced by Temporal Lobe Lesions, Brain **44**:341, 1922.

pressure on the cerebellum by the cerebral tumor. This hypothesis seems to me unsatisfactory. First, it is difficult to believe that a tumor, like that in case 3, which exerted pressure on adjacent structures sufficient only to give rise to weakness of the face and not of the arm, could press on the cerebellum—a structure from which it is separated by the firm, fibrous tentorium—sufficiently to give rise to such pronounced cerebellar phenomena. Second, it is difficult to conceive that this pressure was of such character, especially in cases 1 and 2 as to affect only the cerebellar hemisphere on the opposite side. The pressure by the tumor did not even prove sufficiently intense to affect the adjacent parietal lobe and produce astereognosis in cases 1 and 2. This symptom was present only in case 3, in which the tumor mass involved the parietal lobe directly.

It therefore appears to me far more likely that the "cerebellar phenomena" in lesions of the temporal lobe have their origin in the temporal lobe itself, and that this lobe subserves to a greater or less degree the same functions that are represented in the cerebellum. This supposition as to the functional relationship between these two structures is indicated also by the remarkably extensive anatomic connection between them, by the temporopontocerebellar tracts. The temporal lobe, in accordance with this conception, would form the cerebral or psychic center for the proprioceptive stimuli which, in their ascent to the cerebrum, are relayed first in the cerebellum. The fact that lesions of the temporal lobe give rise to such definite vestibular phenomena as falling to one side, uniform past-pointing and lack of tonus in the muscles, justifies the belief that the stimuli originating in the vestibular portion of the labyrinth form a large component of the proprioceptive stimuli represented in the temporal lobe. The temporal lobe, according to this view, would form the psychic center for the structures of the labyrinth vestibular, as well as of the cochlea. The stimuli from the cochlear division are relayed, as is already established, in the internal geniculate body and the posterior corpora quadrigemina; those of the vestibular portion are relayed in the cerebellum. (The diminution or loss of tendon reflexes on both sides of the body observed in some cases of tumors of the temporal lobe [cases 2 and 3] may be accounted for by bilateral representation in this lobe of the proprioceptive stimuli subserving reflex action, an assumption proposed by Luciani with reference to the auditory stimuli centering in this lobe.)

This view would also explain the absence of nystagmus in lesions of the temporal lobe, regardless of the presence of other cerebellar phenomena. Nystagmus is composed of two components, a slow, primary component, represented by conjugate deviation of the eyes, and a quick component, the return jerk of the eyes to their primary position.

The primary component originates, as discussed elsewhere,[22] in the brain stem, most probably in the nuclei of the third and sixth nerves and their junction by way of the longitudinal bundle. The reflex is consequently not affected by a lesion of the suprasegmental center represented by the temporal lobe, just as the pupillary reflex to light (the center of which is in the anterior corpora quadrigemina and nucleus oculomotorius) is not affected by a lesion of the occipital lobe.

It may be well to point out that a cerebral center for muscle tonus has been presumed to exist by a number of previous writers. The assumption was based on the fact that certain cases of hemiplegia are associated not with spasticity, but with flaccidity of the affected muscles. In 1916, Mills [23] discussed this subject, but the location of this center was not known. I now offer the hypothesis that the center for tonus is situated in the temporal lobe, this lobe forming the cortical center for the labyrinthine tonus of Ewald.

The theory postulated here as to the function of the temporal lobe would also furnish an explanation for the profound and characteristic mental changes that are associated with lesions of this lobe. It is worthy of note that a focal lesion of no other part of the brain gives rise to such profound mental deterioration as does a lesion of the temporal lobe. The patient with a temporal lesion is confused and disoriented; he acts as if he were in no communication with his surroundings. As the lesion progresses, his relationship to the outside world diminishes as if he were becoming separated from it by an ever widening space. If subject to attacks of petit mal, he frequently experiences the illusion, described by Hughlings Jackson, that objects close to him are "far, far away." Bianchi [24] goes so far as to include mental retardation from lesions of this lobe with the insanities. It is at the same time to be borne in mind that the destruction of no other peripheral nerve gives rise to such profound disturbance of function, in its bearing on the patient's consciousness of self and the world around him, as destruction of the vestibular nerve. This fact impressed itself forcibly on my mind when, during the year 1921 to 1922, I carried out, in association with Drs. Isaac H. Jones and Samuel D. Ingham, a series of experiments on the rotation reactions of a cat when projected into the air or dropped from a height (i. e., its capacity to turn around from an original position on its back with the feet up and land on its feet) in the normal state and after lesions were present in the cerebellum and the labyrinth. We recorded our studies

22. Meyers, I. Leon: Nystagmus: Neuro-Otologic Studies Concerning its Seat of Origin, Am. J. M. Sc. **169**:742 (May) 1925.

23. Mills, Charles K.: Some Clinical Studies of the Problem of Cerebral Tone, J. A. M. A. **67**:1485 (Nov. 18) 1916.

24. Bianchi, Leonardo: Text-Book of Psychiatry, trans. by James K. Macdonald. New York, William Wood and Company, 1906, p. 859.

by means of the ultrarapid (so-called "slow") motion pictures. Animals deprived of the labyrinth showed a striking disturbance of function. When the lesion is bilateral, the animal swings its head like a pendulum from side to side, continually searching and groping in space; in walking, it falls from one side to the other; it cannot maintain its head in one position long enough to grasp food; it cannot seize food with its paws, and projects them away from the point aimed at, falling at the same time to the side or backward. In jumping from a table, it falls like a dead mass. This disturbance of function is even more marked when the animal is blindfolded. Profound perturbation of function must therefore necessarily ensue from a lesion of the temporal lobe, which I regard as the suprasegmental center of the vestibular nerve. In addition to depriving the patient of the vestibular stimuli, such a lesion takes away from him more or less the visual stimuli so necessary for proper orientation, as shown by the hemianopic defects.

CONCLUSIONS

1. The cerebellar phenomena in lesions of the temporal lobe are: falling to one side, past-pointing in a definite direction, diminution or loss of tendon reflexes in the limbs, chiefly contralateral, and tremor in these limbs. The first three symptoms are believed to be brought about by interference with the proprioceptive stimuli which pass to the cerebellum from the vestibular apparatus.

2. To explain the vestibular phenomena, the view is here advanced that the temporal lobe is the psychic center for vestibular stimuli and bears the same relation to the vestibular portion of the labyrinth that it does to the cochlear division of this organ.

3. Lesions of the temporal lobe, while giving rise to the cerebellar phenomena enumerated, do not give rise to nystagmus unless they are so extensive as to involve the crura cerebri, the reason being that nystagmus is dependent on interference with the reflex arc of the vestibular system, the center of which is in the nuclei of the ocular nerves in the midbrain and pons. This reflex arc remains unaffected by a lesion which involves only the temporal lobe, the suprasegmental or psychic center of the vestibular system according to my view.

4. The severe mental disturbances observed accompanying lesions of the temporal lobe may be assumed to be an expression of the profound disorientation in space, and the loss of appreciation of one's relation to the environment, which experimental evidence shows results from loss of the proprioceptive stimuli from the labyrinth. If this loss is combined with loss of vision, as shown by hemianopic defects, the patient is more or less completely deprived of the two major elements that enter into adjustment of the self to the outer world.

DISCUSSION

Dr. S. D. Ingham, Los Angeles: The medical literature is surprisingly silent on the cortical cerebral localization of the vestibular functions, probably because the attention has not been focused on disturbances of these functions in terms of the cerebrum. Such symptoms have been taken for granted as being traceable to cerebellar structures, and I think for that reason we should give credit to Dr. Meyers for calling our attention to definite symptoms which usually are considered to be cerebellar but which we all see occasionally in connection with cerebral lesions. The functions and reactions of the vestibular apparatus are manifested in terms of motor coordinations, muscle tonus, nystagmus reactions and certain subjective phenomena, particularly in the nature of vertigo as well as normal sensing of motion.

The three cases that Dr. Meyers has reported showed disturbances of these functions. It seemed reasonable that we should have a cortical representation for so important an apparatus as the vestibular. Further observations will no doubt confirm or disprove this theory.

Dr. Meyers' conception of the so-called vestibular nystagmus differs from my own. I think that vestibular nystagmus, both the slow and the quick component, is part of a special pattern reaction, an automatic combination of movements activated by structures in the brain stem. This pattern reaction is in some measure controllable by cerebral influence or voluntary efforts. In a measure it might be compared to the respiratory movements, which are a series of reciprocal, coordinated pattern movements of a reflex or automatic character, under voluntary control to a limited degree. I think nystagmus should be classified in the same way; both the slow and the quick components are activated by a supranuclear coordinating apparatus of the brain sterm, probably located in the reticular formation and distributed by the posterior longitudinal bundle and other avenues to cells of motor peripheral nerves concerned in the movement.

Dr. A. B. Magnus, Chicago: The mental symptoms described as a result of the tumor in the temporal lobe are quite interesting, if true. It must be remembered, however, that an extensive tumor in any distant area of the brain may produce mental symptoms by indirectly pressing on the frontal lobes.

In 1922, Dr. Woolsey at the Neuropathologic Laboratory of the Harvard Medical School conducted a series of experiments on the vestibular apparatus of pigeons, namely, on the labyrinth, and to quote him, "The impulses originating from the semicircular canals are conducted along the vestibular nerve to the vestibular nucleus, communicating with the nuclei of Deiters and Bechterew and the cerebellum and thence with the posterior longitudinal bundle, ultimately ending in the temporal lobe; the third, fourth and sixth cranial nerves communicating with the posterior longitudinal bundle. This path of connections is nothing but a reflex arc and a lesion anywhere along this path will upset the general normal function and will cause the abnormal findings."

I would like to have Dr. Ayer state more on the subject because he, too, is acquainted with Dr. Woolsey's work. We should accordingly have a nystagmus because of the three nerves mentioned connecting along the main path. Dr. Woolsey also describes a head nystagmus in connection with the eye nystagmus in lesions of this mechanism.

I would ask Dr. Meyers to explain more specifically the mental observations in the case reported, that is, as to whether these were not indirect symptoms of a lesion elsewhere interfering with the frontal lobes.

Dr. James B. Ayer, Boston: I have had occasion to see many abscesses of the temporal lobe in connection with cases of disease of the ear, and one of the supposedly important points in favor of diagnosis of lesions of the temporal lobe has been the absence of cerebellar symptomatology. Although I have not statistics to give, my recollection is that few abscesses of the temporal lobe give the symptomatology which Dr. Meyers' tumors of the temporal lobe have given. I would like to ask whether there were any cases of abscesses in his series in which the symptomatology agreed with his tumor symptomatology?

DR. I. LEON MEYERS: Dr. Ingham's conception that nystagmus is a reflex which involves the quick as well as the slow phase and like the respiratory reflex involves inspiration as well as expiration, is interesting. However, there is evidence against this view. In my paper on "Nystagmus," which was published in the *American Journal of the Medical Sciences* in 1925, it was shown that my studies contained cases of hemiplegia in which the motor cortex was destroyed and there was the conjugate deviation of the eyes and the quick component was absent to the opposite side; the slow component was present but was somewhat modified. Apparently my conception, which is in accord with the prevailing opinion, that the slow component originates in the brain stem and the quick component in the motor cortex of the cerebrum and is a correcting movement, is the one to adhere to at the present time, and if there is no slow component in cases of tumor of the temporal lobe there is clearly no involvement of the reflex arc, and there cannot be the quick component following it.

With reference to head nystagmus, I have had several cases in which this was present, but I am still in the dark as to its causation. Dr. Mygind of Stockholm maintains that such nystagmus is functional. Dr. Bogen and I saw a patient with head nystagmus which was, without a doubt, caused by an organic lesion of the brain.

I have seen a number of cases of abscesses of the temporal lobe in which cerebellar phenomena were present. In these abscesses there is a fluid pressure with an extensive inflamed area around it; this somewhat modifies the picture. In most patients with abscesses of the temporal lobe, it was extremely difficult to determine whether or not there was any past-pointing, as they were drowsy and apathetic. A case of late subdural hemorrhage first drew my attention to the cerebellar phenomena accompanying lesions of the temporal lobe. I thought the patient had a cerebellar lesion, but at necropsy it was found that he had a large hematoma which involved the temporal lobe.

ELECTRICAL SKIN RESISTANCE IN NORMAL AND IN PSYCHOTIC SUBJECTS

DATA SECURED IN CONNECTION WITH PSYCHOGALVANIC STUDIES OF EMOTIONAL REACTIONS *

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In connection with psychogalvanic studies of emotional reactions reported elsewhere by one of us (H. C. S.),[1] measurements were made of the electrical resistance offered by the body to an imperceptible, constant, galvanic current. This paper deals with these data on resistance compiled from 161 records taken on 126 persons, of whom 87 were normal and 39 were of psychopathologic reaction types. The skin resistance was measured before and after each psychogalvanic record. During the test a standard list of about 100 words was read aloud to the subject, and several other auditory stimuli (handclap, automobile horn) and tactile stimuli (pin pricks) were presented. The whole experiment lasted from thirty to fifty minutes. Richter,[2] studying the skin resistance under various conditions (during normal sleep, after injection of drugs, etc.) in both normal and pathologic subjects, has been interested especially in the physiologic factors that determine this phenomenon. It is not our purpose in the present report to discuss the factors underlying the resistance phenomena, but rather to present and to compare the observations of the average resistance made on different groups of normal and psychopathologic persons, all of whom were subjected to a uniform experimental situation.

MATERIAL AND TECHNIC

With few exceptions, our subjects were those whose records were included in the previous reports on the psychogalvanic reflex by one of us (H. C. S.).

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1. Syz, Hans C.: Psychogalvanic Studies on Sixty-Four Medical Students, Brit. J. Psychol. **17**:54, 1926; Observations on the Unreliability of Subjective Reports of Emotional Reactions, ibid. **17**:119, 1926; Psychogalvanic Studies in Schizophrenia, Arch. Neurol. & Psychiat. **16**:747 (Dec.) 1926.

2. Richter, Curt P.: The Significance of Changes in the Electrical Resistance of the Body During Sleep, Proc. Nat. Acad. Sc. **12**:214, 1926; A Study of the Electrical Skin Resistance and the Psychogalvanic Reflex in a Case of Unilateral Sweating, Brain **50**:216, 1927. The Electrical Skin Resistance, Diurnal and Daily Variations in Psychopathic and in Normal Persons, Arch. Neurol. & Psychiat. **19**:488 (March) 1928; Physiological Factors Involved in the Electrical Resistance of the Body, to be published.

Since the subjects were selected with especial care, our groups were fairly homogeneous. Two groups of normal persons were studied. The first was composed of forty-three persons (twenty-five physicians, of whom fifteen were male and ten female, and eighteen other normal persons, nine male and nine female; all subjects were between 20 and 45 years of age); the second group consisted of fifty-four medical students (forty-nine male and five female), all in the third trimester of the first year's work. In addition, there were three groups of psychopathologic reaction types: paranoid schizophrenia, catatonic stupor and depression. In the group of subjects with paranoid schizophrenia (fourteen male and five female, all between 15 and 35 years of age, except one who was 54), the disorder was of only a few months' standing in most cases; at the time of the experiment, the psychotic condition presented marked ideas of reference, feelings of influence, delusions of persecution and hallucinations (voices). The catatonic patients (three male and three female, aged from 20 to 29 years) were all, at the time of the tests, in a condition of stupor, showing little or no response, some with catalepsy. Of the cases of depression (eleven male and three female patients, ages 22 to 29 years) some were of the recurrent type and on a constitutional background, and others more of the reactive type; in addition to the definitely depressive mood these patients presented symptoms such as hypochondriac preoccupation and, delusions, obsessive features, feelings of unreality and ideas with paranoid coloring. Agitated depressions were excluded.

Since the technic of the experiments in which the resistance readings were obtained has been described elsewhere by one of us (H. C. S.),[3] and since the method of measuring the resistance has been discussed fully by Richter,[4] only a few of the principal points need be mentioned here. The resistance readings were taken with an Einthoven string galvanometer (Hindle model) according to the technic employed for a number of years in the psychobiological laboratory of the Henry Phipps Psychiatric Clinic. Nonpolarizable electrodes, consisting of zinc disks about 1¼ inches in diameter and one-half inch thick, covered with a paste of kaolin and saturated zinc sulphate solution, were applied to the back and palm of each hand and attached by brass rods to arm rests especially constructed for the purpose.

By means of four different pairs of terminals, used successively, the subject was introduced into the circuit so that the current flowed first through the right hand from back to palm, then through the left hand, then from the back of one hand to the back of the other and finally from the palm of one hand to the palm of the other. Thus each reading is the sum of the resistances of two areas of skin, and the sum of the resistance of all four areas of skin is the same, whether the current passes through only the hands (right or left) or through a much greater part of the body (back-to-back or palm-to-palm), the resistance of the body tissues being negligible. This method, developed by Richter, to differentiate the resistance of the backs and the palms, makes it possible to investigate the behavior of each of these areas in the psychogalvanic response and associated resistance phenomena.

Since a direct current (1 milliampere) was employed in these experiments, our values give the "apparent" and not the absolute resistance. The galvanometer string was standardized for the resistance reading so that the introduction of 1 millivolt into the circuit caused a 10 mm. deflection of the shadow of the string. With the subject in circuit a higher voltage was needed to deflect the

3. Syz (footnote 1, first and third references).

4. Richter (footnote 2, second and fourth references).

shadow 10 mm. From this voltage (V) and from the known resistance (X) of the galvanometer string, the resistance (R) of each pair of skin areas can be derived by the formula: $R = X\ (V-1)$.

RESULTS

Observations on Resistance.—In certain features, the skin resistance presented the same picture in all of the groups examined: 1. In practically all cases the resistance of the backs of the hands was much higher than that of the palms, in agreement with Richter's work. 2. In the majority of cases the resistance of the right hand was higher than that of the left. 3. A decrease in resistance usually occurred during the period of the experiment.[5] Although in these features there were individual variations, the observations as a whole were definite. In a group of sixty-four normal persons (including the group of medical students), only two subjects showed a higher resistance of the palms than of the backs; a comparison of the two hands showed that in 71 per cent of the records the resistance of the right hand was higher, in 7 per cent the resistance of the two hands was equal, and in 22 per cent the resistance of the left hand was slightly higher than that of the right. In 14 per cent of the cases, there was a slight increase in resistance during the test, and in 16 per cent there was no change; 70 per cent showed a decrease. In the three psychopathologic groups there were only two cases in which the resistance of the palms was not lower than that of the backs, and only two records which did not show a decrease in resistance during the test.

Besides these features, which were in general consistent for all persons examined, there were aspects especially characteristic of the different reaction groups. As may be seen in table 1, the resistance values for each hand for the catatonic and depressive groups are far greater than those for patients of the paranoid schizophrenic group and for normal persons. The back-to-back resistance for the former groups is extremely high, whereas the palm-to-palm readings do not differ markedly from the values obtained for the other groups. It may be seen from the table that these group differences hold for the readings taken after the test as well as for those that precede it. This difference between the resistance of the backs and that of the palms—especially outstanding in catatonic patients—was first observed by Richter in 1923, and has been reported by him.[6] The data of his study are based on records taken on each of several persons on successive days over rela-

5. This decrease is not the same as the "curve of rest," reported by Veraguth, since in the present study an external current was used only while the resistance records were being taken and no external current was used during the remainder of the experiment.

6. Richter (footnote 2, third and fourth references).

tively long periods of time. The results which we present here, however, are averages for groups of persons whose resistance readings were taken on only one or two occasions. Although we would expect greater individual fluctuations from results obtained by the latter method, the characteristic features of the group averages are unquestionable.[7]

Also, in the decrease of resistance during the test period, there are conspicuous group differences. These are shown in table 2. Because the initial resistance varies from group to group, the change in resistance has been calculated as percentage of the resistance before the test. Although the decrease is present in all groups, it is especially pronounced in the paranoid schizophrenic group (average drop for all four readings 46 per cent) and least marked in the depressive (average drop 25 per cent). This difference in the change of resistance level appears more clearly when the palm-to-palm resistance is considered alone, for in the paranoid schizophrenic group the decrease is 35 per cent of the initial reading, whereas in the depressive and catatonic groups it is only 8.9 and 9.5 per cent, respectively. As the initial palm-to-palm resistance is about the same for all five groups, the discrepancy in the decrease is the more interesting.[8]

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7. The ratio between the resistance of the backs of the hands and that of the palms, which Richter found to average 3.7 for normal persons and to be much higher for depressive and catatonic persons, was here also much lower for the normal and schizophrenic groups than for the others, though there was more variation in the normal groups than in Richter's data (possibly due to the fact that the readings of each subject were for one experimental period only). The lowest ratio for the catatonic and depressive group averages was 7.6 and the highest ratio in the normal groups was 6.7, while for individual catatonic patients it ran as high as 38.

8. Thouless, Robert H.: The Causes of the Continuous Change of Resistance Observed in Psychogalvanic Experiments, Brit. J. Psychol. **16**:5, 1925. Thouless, who concludes from his experiments that the resistance changes cannot be explained as polarization phenomena, suggests that the "mental adjustment of the subject" is an important factor in the decrease in resistance and that "the condition of low resistance appears to be one of preparedness to react to a stimulus (alertness), while the condition of high resistance is one of unpreparedness to react (a condition which culminates in the state of sleep)." Our observations, however, suggest that the situation is more complicated, for, though low initial resistance and marked decrease in resistance in our groups were, on the whole, associated with greater psychogalvanic responsiveness, the responsiveness was in general less toward the end of the test (when the resistance was lower) than at the outset when the resistance was relatively high. Also from our observation of the persons under test and from their subjective reports we gathered that, as a rule, in the beginning of the experiment when the resistance was highest there was more attention and alertness and that later on, when the resistance was lower, there was ordinarily a state of greater indifference.

Table 3 gives the decrease in resistance for the backs and palms directly in ohms as well as computed as per cent of the initial readings. Here it will be seen that the palmar resistance of the paranoid schizophrenic group showed an average drop of 15,300 ohms (35 per cent of the initial resistance), while for the catatonic group the drop was only 4,000 ohms (9.5 per cent of the initial resistance). For the backs of the hands also the percentage drop is definitely more marked in the paranoid schizophrenic than in the depressive group though in these readings the two groups differ less than in the decrease of the palm-to-palm

TABLE 1.—*Resistance (Averages for Groups)* *

	Normal Group 1		Normal Group 2		Paranoid Schizophrenic Group		Depression Group		Catatonic Group	
	Before Test	After Test	Before Test	After Test	Before Test	After Test	Before Test	After Test	Before Test	After Test
Right hand	127	83.4	179	109.7	156.7	80	273.5	184	373.5	200
Left hand	112.7	69	128	80	143.3	73.3	210	154.6	357	193
Back-to-back.	184	111	261	164	250	120	443	300	660	373
Palm-to-palm	53.7	43	38.6	31.6	43.3	28	45	41	41.7	37.7
Average	119	76	151	98	166	75	263	170	360	201

* The figures indicate ohms in thousands

TABLE 2.—*Average Decrease in Resistance in Percentage of First Reading*

	Normal Persons Group 1	Normal Persons Group 2	Paranoid Schizophrenic Patients	Persons with Depressive Conditions	Catatonic Patients
Right hand	35.0	41.2	49.0	32.5	54.4
Left hand	38.4	30.5	48.8	26.4	47.0
Back-to-back	39.6	37.2	52.0	32.5	43.4
Palm-to-palm	18.8	18.4	35.3	8.9	9.5
Average	32.9	31.8	46.3	25.1	38.5

resistance and in the catatonic stupors, there is even a reversed condition—a slight decrease for the palms and a marked decrease for the backs.

The two groups of normal persons take rather an intermediate position. Especially in the drop in the palm-to-palm resistance during the experiment they stand between the extreme values that occur, as outlined, in the pathologic reaction groups.

Some of these data (group differences in average resistance found mainly in the resistance of the backs, not of the palms, and decrease in resistance found to differ for backs and palms) indicate that the resistance phenomena of the back and palm areas are relatively independent of one another. Thus the statistical data here presented agree on the

whole with the observations reported by Richter,[9] who has investigated the physiologic background of the skin resistance and reaches the conclusion that in the palmar and dorsal resistance of the hands we deal with different phenomena. It is his view that the changes in palmar resistance are due essentially to the activity of the sweat glands, while in the resistance of the backs some other mechanism is of primary importance, probably the activity of the epithelial cells with concomitant permeability changes.[10] Our data suggest that the mechanisms determining the resistance of both of these areas (backs and palms) are affected in those disturbances of the entire organism which we designate as "mental disorders," and that the way in which these physiologic part-functions are altered is characteristic to some degree of certain types of personality disorders.

TABLE 3.—*Decrease in Resistance During Test*

	Backs		Palms	
	Ohms, Thousands	As Percentage of First Reading	Ohms, Thousands	As Percentage of First Reading
Normal				
First group	73	39.6	10.7	18.8
Second group	97	37.2	7	18.4
Paranoid schizophrenic group	130	52	15.3	35.3
Depressive group	143	32.5	4	8.9
Catatonic group	287	43.4	4	9.5

COMPARISON OF OBSERVATIONS ON RESISTANCE WITH THE BEHAVIOR OF THE PSYCHOGALVANIC CURVE

Studies of the psychogalvanic reflex by Veraguth,[11] Waller,[12] Prideaux,[13] Wechsler,[14] Gregor and Loewe,[15] and others, have considered the relationship between the reflex and changes in resistance,

9. Richter (footnote 2, first, second and fourth references).

10. Rein, Hermann: Die Gleichstromleiter-Eigenschaften und elektromotorischen Kraefte der menschlichen Haut und ihre Auswertung zur Untersuchung von Funktionszustaenden des Organes, Ztschr. f. Biol. **85**:195, 1926. In this study, which further demonstrates the relationship between electrical phenomena of the skin and permeability changes, the author calls attention also to accessory factors which may be misinterpreted as an alteration of the tissue function.

11. Veraguth, O.: Das psychogalvanische Reflexphaenomen, Berlin, S. Karger, 1909.

12. Waller, A. D.: Periodic Variations of Conductance of the Palm of the Human Hand, Proc. Roy. Soc. **91**:17, 1919.

13. Prideaux, E.: The Psychogalvanic Reflex: A Review, Brain **43**:50, 1920.

14. Wechsler, D.: The Measurement of Emotional Reactions, Arch. Psychol. No. 76, 1925, pp. 25-206.

15. Gregor, A., and Loewe, S.: Zur Kenntnis der physikalischen Bedingungen des psychogalvanischen Reflexphaenomens, Ztschr. f. d. ges. Neurol. u. Psychiat. **12**:411, 1912.

while Gildemeister [16] has emphasized other aspects of skin resistance. The exact interrelation of the two phenomena, however, has not as yet been definitely established, though we know that both are dependent on mechanisms in the skin and its appendices. We would therefore expect to find some relationship in the groups here studied between the behavior of the skin resistance and the reflex response to stimuli. Of the psychogalvanic observations in our groups, reported by one of us (H. C. S.),[17] the following are of interest here: 1. Depressive and catatonic patients showed a relatively low frequency of response in contrast to normal persons and paranoid schizophrenic patients who presented more numerous reactions and in general a more labile psychogalvanic curve.[18] 2. In records from patients of the paranoid schizophrenic group, there were frequent "spontaneous" fluctuations of the galvanometer string, that is, fluctuations which were not directly associated with the stimuli presented. 3. Among normal persons, those of a more "emotional" nature gave a greater number of reactions than those of a more stable and less sensitive type.

A comparison of these peculiarities of the psychogalvanic reactions with the resistance data shows that the relative frequency of responses to stimuli is greater for those groups in which the resistance is lowest (normal persons, paranoid schizophrenic persons) and less for those whose resistance is high (catatonic persons and those of the depressive type). When this correspondence was examined more in detail in the second group of normal subjects, it was found that the fourteen persons with the greatest reactivity (reactions to from 56 to 71 per cent of the stimuli presented) showed a resistance (113,390 ohms) considerably lower than the average for the group (157,360 ohms), with responses to only 35 per cent of the stimuli. Thus it is evident that low resistance is, on the whole, associated with high responsiveness, not only if one

16. Gildemeister, M.: Ueber die physikalisch-chemischen und physiologischen Vorgaenge im menschlichen Koerper, auf denen der psychogalvanische Reflex beruht, München. med. Wchnschr. **60**:2389, 1913; Der sogenannte psychogalvanische Reflex und seine physikalisch-chemische Deutung, Arch. f. d. ges. Physiol. **162**:489, 1915; Zur Physiologie der menschlichen Haut, ibid. **200**:251, 1923.

17. Syz (footnote 1, first and third references).

18. For other studies of psychogalvanic reactions of psychopathologic individuals see: Peterson, F., and Jung, C. G.: Psychophysical Investigations with the Galvanometer and Pneumograph, Brain **30**:153, 1907; Ricksher, Charles, and Jung, C. G.: Further Investigations on the Galvanic Phenomenon and Respiration in Normal and Insane Individuals, J. Abnorm. Psychol. **2**:189, 1907; Gregor, A., and Gorn, W.: Zur psychopathologischen und klinischen Bedeutung des psychogalvanischen Phaenomens, Ztschr. f. d. ges. Neurol. u. Psychiat. **16**:1, 1913; Prideaux, E.: Expression of Emotion in Cases of Mental Disorder as Shown by the Psychogalvanic Reflex, Brit. J. Psychol. med. sec. **2**:23, 1921.

compares the results compiled from the various groups, but also if the records of various persons in the same group are examined.

In order that we might study further the relation of the resistance to the frequency of response, the resistance was artificially lowered in a number of subjects by slight injuries to the skin. Though the resistance fell immediately, there was no marked effect on either the frequency or the amplitude of the reactions. It is of interest that such an artificial lowering of the resistance has practically no effect on the psychogalvanic responses, while sweating lowers the resistance and also alters the reflex.[19] This circumstance suggests that it is not the low

TABLE 4.—*Frequency of Spontaneous Waves in Records of the Psychopathologic Reaction Groups*

	Cases with Less Than 6 Spontaneous Waves		Cases with Frequent Spontaneous Waves		
Number of spontaneous waves on record...	0	1-5	6-30	31-60	61-90
Paranoid schizophrenic group (28 records)..	5	5	6	8	4
Depressive group (14 records).....	9	0	3	2	0
Catatonic group (15 records).....	10	1	3	1	0

TABLE 5.—*Relationship Between Presence of Frequent Spontaneous Fluctuations and Resistance of Palms*

	Resistance of Palms						Drop of Palm Resistance During Test in Percentage		
	Average for Cases with Frequent Spontaneous Waves		Average for Entire Group		Average for Cases with Less Than 6 Spontaneous Waves		Cases with Frequent Waves	Average for Group	Cases with Less Than 6 Waves
	Before Test	After Test	Before Test	After Test	Before Test	After Test			
Paranoid Schizophrenic group.	36.6	21.6	43.3	28	52.4	39.1	40.9	35.3	25.3
Depressive group	28	22	45	41	74.1	70.1	21.4	8.9	5.4
Catatonic group	16.4	9.8	41.7	37.7	50.9	47.9	40.2	9.5	5.8

resistance per se, but the altered physiologic mechanism responsible for the low resistance which is associated with the galvanic reaction.

There is also a relationship between the appearance of spontaneous fluctuations and the degree of resistance. The group of paranoid schizophrenic persons, in whose records spontaneous fluctuations were most

19. Richter has found that a hot air bath results in an exaggeration of the reflex, that the quick phase of the response is associated with the palm resistance, and that spontaneous waves appear consistently with marked sweating. (See also footnote 1, second reference.) Darrow, Chester W.: Sensory, Secretory and Electrical Changes in the Skin following Bodily Excitation, J. Exper. Psychol. **10:** 197, 1927. This study, published since the present paper was written, gives further experimental evidence of the relationship between the psychogalvanic reflex and the activity of the sweat glands.

frequent, presented a relatively low initial resistance and a much greater decrease in the resistance of the palms during the test than was found for the other groups. As spontaneous waves occurred also in a few of the depressive and catatonic patients, and since a few records of the schizophrenic group failed to present this phenomenon (table 4 shows the frequency of spontaneous waves), it was of special interest to compare the resistance in these exceptional cases with the average resistance in their respective groups. These data are summarized in table 5. It was found that persons with numerous spontaneous waves in the psychogalvanic record showed, in general, a lower palmar resistance and a greater decrease in resistance during the test than the average of the group to which they belonged, while those paranoid schizophrenic patients whose records did not show spontaneous fluctuations had a higher palmar resistance and a smaller decrease during the test than was found in the average of the paranoid schizophrenic group.

The frequent occurrence of spontaneous waves, then, seems to be linked up in some way with the resistance of the palms and is thus probably related to the activity of the sweat glands.

SUMMARY

The following features were found consistently throughout the various groups studied here. The back-to-back resistance was relatively much higher than the palm-to-palm resistance; the resistance of the right hand was higher than that of the left, and a decrease in resistance occurred during the period of the experiment.

Among the group differences, the following were conspicuous: 1. Depressive and catatonic patients, giving, in general, few psychogalvanic responses to experimental stimuli and few spontaneous waves, had a high initial resistance, especially of the backs of the hands, and showed a slight change in palmar resistance during the test. 2. Patients of the paranoid schizophrenic type, giving relatively frequent psychogalvanic responses to experimental stimuli and a relatively high percentage of spontaneous fluctuations, had a relatively low resistance for all skin areas and a marked decrease in resistance during the test. 3. The groups of normal persons, who had given rather frequent responses and few spontaneous fluctuations, differed little in initial resistance from the patients of the schizophrenic group, but they showed much less change in resistance during the experiment. Thus in the behavior of the skin resistance and in psychogalvanic responsiveness, these normal groups stand between the paranoid schizophrenic group, on the one hand, and the catatonic and depressive groups, on the other.

These group differences, which appear with marked consistency, indicate that certain alterations in the function of the physiologic

mechanisms of the skin are characteristic for the various reaction types. This is to be expected, since the skin is an organ which is ontogenetically as well as functionally closely connected with the central nervous system. There is every indication that the skin mechanisms underlying the phenomena here studied are influenced through impulses arising in the central nervous system and that thus changes of the entire organism (emotions and psychopathologic conditions) involve characteristic alterations of those physiologic part-functions which determine, or are expressed through, the electrical skin resistance and the psychogalvanic reflex.

QUANTITATIVE STUDIES ON HUMAN MUSCLE TONUS

I. DESCRIPTION OF METHODS *

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Methods for the quantitative study of human muscle tonus have been developed along two different lines: 1. Several mechanisms have been designed to give a measurement of the resistance of muscles to pressure on the muscle bellies. There can be little doubt that there is a certain correlation between the hardness of muscles and their ability to resist a stretching pull, but tonus is usually thought of in terms of resistance to stretch so that these methods are obviously indirect unless one defines tonus to mean muscle hardness. At any rate, studies of this sort appear, on the face of them, subject to considerable error because of the necessity of considering the compressibility of extraneous tissues involved in the measurements. No one appears to have followed these methods long enough to have presented a searching analysis of normal muscle compressibility.

2. Several writers have published descriptions of machines with which the resistance of muscles to longitudinal stretch can be estimated in a variety of ways. Spiegel [1] and Filimonoff [2] have evaluated the merits and demerits of the machines described previous to theirs. Filimonoff published a method subsequently to that of Spiegel and criticized Spiegel rather severely. Filimonoff's idea was a good one in principle, but he depended on the ability of himself and his assistants to produce a constant pull by hand against a recording tambour. He himself states that he discarded tracings that were "grossly irregular." Kuntz and Kerper [3] have undertaken some studies in connection with

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* From the Division of Nervous and Mental Diseases, University of Minnesota Medical School. We are indebted to Mr. John T. Tate, Professor of Physics at this institution, for the elucidation of several principles of physics in connection with this article which were not clear to us and which required explanation before a satisfactory method of procedure was possible.

1. Spiegel, E. A.: Zur Physiologie und Pathologie des Skelettmuskeltonus, Ztschr. f. d. ges. Neurol. u. Psychiat. **81**:517, 1923.

2. Filimonoff, I. N.: Klinische Beiträge zum Tonusproblem, Ztschr. f. d. ges. Neurol. u. Psychiat. **96**:368, 1925.

3. Kuntz, A., and Kerper, A. H.: An Experimental Study of Tonus in Skeletal Muscles as Related to the Sympathetic Nervous System, Am. J. Physiol. **76**:121, 1926.

sympathectomy, using Spiegel's machine. One objection to Spiegel's method is that the weight of the extremity (leg) distal to the joint (knee) must be counterbalanced by a weight added to the machine. According to Spiegel's figures, taken from Braune and Fischer,[4] this introduces an error of ± 7 per cent approximately; moreover, the measurements of Braune and Fischer were made on cadavers selected for study because of their similarity in good muscular and bony development and their absence of excessive adiposity. In any run of unselected cases this error must become considerably greater. Then too, Spiegel does not describe his technic of balancing out muscle tonus in sufficient detail to give knowledge of the speed of application of weights for this purpose, or of the time elapsed between the application of the weight and the reading of the angle; presumably, therefore, he has not considered this as a possible source of error. This is important because the plasticity of muscle makes comparative readings of resistance to stretch seriously variable with variations in the time interval and the speed of application of the weight. This fact can be amply verified in the existent literature on the experimental physiology of muscles.

Kuznetsov[5] cleverly took advantage of the sesamoid nature of the patella, using it as an anchor in the tendon against which known forces could be applied to measure changes in the resistance to stretch of the quadriceps muscles. He studied particularly the shortening and lengthening reactions to different stimuli under the pull of a constant weight. His method is applicable only to the quadriceps femoris.

Up to the present we have limited our studies to the tonus of the extensors and flexors of the elbow joint. There is no special reason other than convenience for selecting this joint. While the machines which we have constructed are designed primarily for the examination of this joint, there is no reason why, with slight modifications, they may not be adapted to the study of the wrist, finger, knee and ankle joints.

We have developed several methods, more or less similar in principle, for the quantitative recording of the pull of the resting muscles around the elbow joint in flexion or in extension. With the first machine it is possible to measure in a short period of time the physiologic resistances around the joint during passive flexion or extension. The second method determines the tonus torque in terms of an angle at which the arm will come to a balance under the influence of a certain torque

4. Braune, W., and Fischer, O.: Ueber den Schwerpunkt des menschlichen Körpers, Abhandl. d. kön. sächs. Ges. d. Wissensch. 26, math. phys. Classe **15:** 559, 1890.

5. Kuznetsov, V. P.: A New Graphic Method of Examination of Muscle Tonus in Man, Vrachebnoie Dielo, Kharkov **8:**457, 1925. From a translation of this article by the Consulting Bureau, Tice Digest, Chicago.

on the apparatus, and at which, therefore, the muscle tonus torque equals the machine torque. The third method traces a curve of the increasing torque of the muscles under the steady pull of a force applied through reducing gears from a motor.[6] Our purpose in developing more than one method of measurement has been to check the methods against one another and thus to demonstrate the relative reliability of each; also to apply forces at different known speeds so as to ascertain the physiologic effect of this factor; and, as well, to observe whether or not it is possible to develop a simple measure which will be satisfactorily accurate for ordinary diagnostic utility. Our studies are not far enough advanced, however, to permit a definite conclusion on this latter point.

In each of these cases we have placed the arm in such a position that the elbow joint, as far as flexion and extension are concerned, is freed from the effect of gravity except as we apply gravity through weights on the machine. Consequently, the flexion and extension are performed in the horizontal plane. In addition to freeing the arm from the effect of gravity, the problem has been to make the moving parts of the machine as light and frictionless as possible and at the same time to preserve marked stability for the avoidance of oscillation and play in the machine. Also we found it requisite to make the apparatus as noiseless as possible, at least as regards that part of the operation which deals with a quick measure of tonus.

DESCRIPTION OF THE APPARATUS

The patient is placed semirecumbent in a dental chair with the trunk and head of the body at about a 60 degree angle from the horizontal. The height of the chair is so adjusted that the shoulder is higher than the arm rest (*r*) of the machine and so that with the forearm and hand on the arm rest the upper arm forms an angle of approximately 10 degrees with the horizontal. Variations up to 5 degrees in the angle of the arm from the horizontal appear to introduce no error in the curves. The chair is so placed in relation to the machine that the arm is as nearly as possible in the plane of abduction, that is, drawn neither anteriorly nor posteriorly. The latter position is important because retraction of the arm increases the tension on the biceps and decreases the tension on the triceps muscles, and a position too far anteriorly does just the reverse. The arm rest of the apparatus rotates freely on a ball bearing spindle (*b. b. sp.*), the stationary steel axis rod (*a. r.*) of which passes through the center of a kymograph drum and fits over a peg (p_2.) protruding upward from the table top. Attached to the lower end of the spindle and rotating along with the arm rest is the main fiber disk (*m. f. d.*) which is grooved around its circumference so as to hold a string or wire. The string can be attached to the circumference of the disk by means of peg (p_1) fastened to the disk in an upright position at the circumference. Another smaller fiber disk (*s. f. d.*) projects out from the under

6. Because of the amount of writing necessary to describe clearly the details of our methods we shall limit the present paper to a consideration of the first apparatus mentioned.

side of the larger disk in the direction opposite to that of the arm rest, and carries a perpendicular metal cylinder (*m. c.*) on which is attached an electromagnetic time marker (*t. m.*) in adjustable positions. The time marker can be so placed as to write on the smoked paper of the kymograph drum. Thus, the drum is stationary while the arm rest and time marker rotate horizontally about it on the same axis. From the peg on the circumference of the disk (p_1), strings are led around the circumference in opposite directions each over a pulley at the edge of the table. Each string then runs downward to its respective brass rod which is 4 mm. in diameter and 40 cm. in length. The rods are practically equal in weight (42.5 Gm. and 42.7 Gm. respectively) and thus counterbalance one

Fig. 1.—Tonus apparatus ready for the recording of a tracing in passive flexion.

another. The rods each pass through a trip mechanism, the essential part of which is a small metal cylinder rotating eccentrically so as to clamp tightly on each rod separately or to release it quickly (fig. 1). Weights are hung on the free ends of the brass rods as desired. Thus by hanging equal weights on each brass rod there will be no tendency to turn the elbow in either flexion or extension, but in the case of putting a weight on one and leaving the other one free the arm will be turned in one or the other direction. The kymograph drum is securely held to the table by two wedge-shaped clamps which can be turned over the inner flange around the bottom of the inside of the drum by turning a thumb

screw under the table top. On each leg of the table is a leveling screw so that we can accurately place the arm in the horizontal plane. The time marker is wired through the drum to a battery and an electric tuning fork running at a speed of 50 interruptions per second. An electric key is also in the circuit so that the marker can be stopped as quickly as desired. (Figure 2 illustrates the wiring diagram.)

TECHNIC OF OBTAINING THE TRACINGS

In obtaining a tonus reading with the apparatus, the patient is placed in the position already described with the forearm held against the outer bar of the arm clamp near the elbow. The hand is held down by a strap passing over the metacarpal region with the fingers in a flexed position so that the hand rests on the second phalanges. The two strings are passed around the disk and each runs over a ball-bearing wheel at the edge of the table. The brass rods are in place through the trip mechanism. Two equal weights are hung, each on one of the

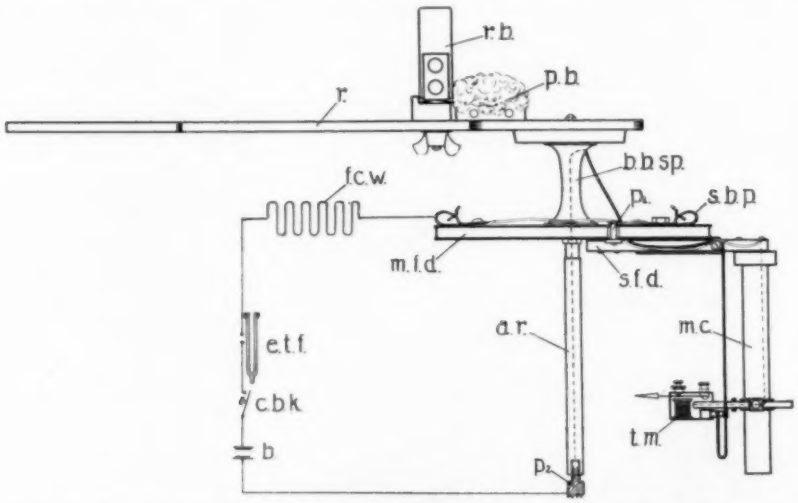

Fig. 2.—Head of the apparatus and wiring diagram. *r* indicates the arm rest; *b.b.sp.*, ball bearing spindle; *a.r.*, stationary steel axis rod; p_1 and p_2, peg; *m.f.d.*, main fiber disk; *s.f.d.*, small fiber disk; *m.c.*, metal cylinder, and *t.m.*, electromagnetic time marker.

brass rods, so that there is no tendency for the arm to be pulled in one or the other direction. With such an arrangement it is found that practically all normal arms assume nearly the same position, with the forearm at approximately a 90 degree angle from the arm. After the forearm has taken this 90 degree angle at rest the trip mechanism is clamped down on the brass rods, thus holding the arm in the neutral position. Then one of the weights is removed (from one of the brass rods if one desires to study the movement of flexion, from the other if extension is desired). The clamp of the trip mechanism on the side where the weight is removed is left in the clamped position, but the clamp for the trip mechanism where the weight is still on the brass rod is released. This "setup" is shown in figure 1. The arm does not change its position in this case because the clamp has been substituted for the weight that was removed, and the pull has not been changed because the original weight on the other side of the

mechanism is still acting. All that is necessary then is to see that the time marker is playing against the kymograph paper, that the tuning fork is running and that the patient is relaxed. The trip mechanism which is still clamped is then quickly, but carefully and quietly, released. The weight thus being released turns the arm either in flexion or extension according to the original choice. The time marker traces a time curve of the movement in fiftieths of a second. At the end of the curve, the time marker is stopped quickly by pressing on the key to break the circuit. The circumference of the drum with the paper on it is 720 mm., and since, therefore, an angle of one degree corresponds to 2 mm. of linear measurement along the circumference of the drum, the speed of the arm at any given angle can readily be obtained from the curve. Several curves are taken at one sitting, and we usually follow such an order for flexion and extension, and for the application of different weights, that the patient does not know whether his arm is about to be flexed or extended or how much weight is about to move his arm. Preliminary to taking the first reading, we always instruct the patient exactly as follows: "Please relax as completely as you can. Do not think of anything but relaxation. Your arm may be drawn out or pulled in, and it will be activated by different weights giving it various speeds, so do not try to interfere with the machine but relax completely." Each time a subsequent curve is obtained the patient is asked to relax as completely as he can. The patient is then released from the machine and allowed a few minutes rest. The chair is turned around and the other arm is examined. The same technic is followed out as previously described.

CALCULATION OF THE TONUS TORQUE

It is obvious from the foregoing description that, with the patient's forearm on the arm rest and during the taking of a tracing, there are two torques acting on the apparatus. One is the resultant of the torques of the individual muscles acting around the elbow and tending to flex or extend the forearm according to the action of each muscle. This resultant torque is the quantity which we desire to evaluate. We shall call it T. The other is the torque of the apparatus itself, which is arbitrarily determined by the weight used to turn the arm through the radius of the disk on which it acts. We shall call this torque T_o. Aside from these torques, the time curve is influenced by the moment of inertia of the machine (I_o), the moment of inertia of the forearm and hand of the patient (I_s), and the moment of inertia of the driving weight (I_m). The angular acceleration of the forearm which is being pulled out by the force of gravity on the driving weight will be represented by a. Then the equation of motion of the apparatus with the patient's arm in it is:

$$T_o - T = (I_o + I_s + I_m)\, a$$

or

$$T = T_o - (I_o + I_s + I_m)\, a \qquad \text{Equation 1.}$$

In order to obtain the value of T, it is necessary to evaluate each of the quantities on the right hand side of the equation. There is no difficulty in making these calculations except for the determination of the moment of inertia of the forearm and hand of the patient. It is

obvious from data on the specific gravities of the various tissues making up the forearm and hand (Vierordt[7]), that there is opportunity for considerable variation in moments of inertia due to the variability in fat, bone, muscle and other tissue distributions in the extremities. For instance, a fat person has relatively a small moment of inertia as compared with a thin, heavily boned person, even though the volumes of their two forearms are the same. Recognition of this fact resulted in our studying four cadaver arms in some detail in order to determine the magnitude of the error introduced into the calculations when evaluating the muscle tonus torque by means of equation 1. We selected the four arms so as to obtain as large a range of variability of specific gravities as possible. One was small and fat; one muscular with small bones and little fat; one was heavily boned, muscular and lean; and one was heavily boned but emaciated. It appeared from this study that the moment of inertia of the arms varied about a mean value by ± 7 per cent approximately. This in itself is not a serious error considering the marked variation present in the value of the tonus torque from subject to subject and in the same subject from time to time. However, it is evident from the equation that in calculating the tonus torque (T), the sum of the various moments of inertia, and consequently the sum of any errors in the moments of inertia, are multiplied by the value of the acceleration, so that the error assumes considerable proportions, sometimes going as high as 50 per cent of the value of T.

Inspection of equation 1 shows that if we can select a point in our tracing where there is no acceleration, or where a equals zero, the product of the sum of the moments of inertia times the acceleration will be equal to zero and the equation will become

$$T = T_o \qquad \text{Equation 2}$$

thus ridding us of all consideration of the moments of inertia.

Before discussing the location of the point in the curve where equation 2 is applicable, we shall state the meaning of T_o. The physicist (for example Crew[8]) defines torque as the "product of a force times the perpendicular distance from the axis to the direction of the force." Again, "force is the product of the acceleration which it produces in a body multipled by the mass of the body." Consequently, for the special case of our machine

$$T_o = MgR \qquad \text{Equation 3}$$

in which T_o is the torque driving the machine, M is the number of grams of weight arbitrarily used to turn the arm rest, g is the value of

7. Vierordt, H.: Anatomische, physiologische und physikalische Daten und Tabellen zum Gebrauche für Mediziner, Jena, Gustav Fischer, 1906.

8. Crew, H.: General Physics, New York, The Macmillan Company, 1916.

the acceleration of gravity in centimeters per second per second (about 980 in Minneapolis), and R is the radius of the fiber disk (10.42 cm. in the case of this particular apparatus).

Thus, if we use a weight of 500 Gm. to drive the machine, equation 3 becomes

$$T_o = 500 \times 980 \times 10.42 = 5{,}105{,}800$$

Similarly, for a weight of 750 Gm.,

$$T_o = 750 \times 980 \times 10.42 = 7{,}658{,}700,$$

and for a weight of 1,000 Gm.,

$$T_o = 1000 \times 980 \times 10.42 = 10{,}211{,}600,$$

and so on for any weight under the action of gravity which we might select as the motive force.

These values of T_o must have subtracted from them the value of the friction in the machine. We have determined that, with a moment of inertia on the arm rest corresponding to a large forearm and hand (about 800,000), 7 Gm. used as the motive force will just keep the arm rest turning at a constant speed. Consequently, the amount of T_o used up by friction in the machine is

$$7 \times 980 \times 10.42 = 71{,}481, \text{ or approximately } 71{,}500.$$

Hence, in the expression $T = T_o$, the value of T_o, with a 500 Gm. weight driving the machine is actually

$$5{,}105{,}800 - 71{,}500 = 5{,}034{,}300;$$

with a 750 Gm. weight, it is

$$7{,}658{,}700 - 71{,}500 = 7{,}587{,}200,$$

and with a 1,000 Gm. weight, it is

$$10{,}211{,}600 - 71{,}500 = 10{,}140{,}100$$

and so on for other weights.

No doubt, with a large arm on the arm rest, there is more friction than with a small arm. On the other hand, running the machine alone without any weight placed on the arm rest to give an additional moment of inertia, 5 Gm. is the weight which will just keep the arm rest rotating at a constant speed. Consequently, the discrepancy is so small in regard to varying frictions with different weights of arms on the machine, that it can be disregarded without further comment.

Inspection of tracings of a patient's arm shows that if the driving weight is not too large, the arm is pulled out with a positive acceleration for the first part of the tracing; that after this period of positive acceleration, the tonus torque becomes great enough to produce a deceleration, and finally enough to stop the fall of the weight altogether. Compare

with figure 3, tracing 1, for example. In other words, at the first part of the tracing the torque of the machine and weight is greater than the torque of muscle tonus, and during the latter part of the tracing the torque of muscle tonus is greater than that of the machine and weight. During any period when the angular velocity is unchanging (no positive or negative acceleration), or at any point where positive acceleration changes more or less abruptly to deceleration, the torque of muscle tonus equals the torque of the machine and weight; that is, T will equal T_o at this point. By measuring the length of the individual waves in the tracing it is a simple matter to locate this period of time or this point in the tracing, because the location will fall within the longest oscillation (one fiftieth of a second) of the time marker, or correspond to the longest oscillations if several of them are equal.

In measuring the tracing to determine the longest oscillation, we place the tracing under a strip of transparent celluloid for protection against scratches and compare the length of the individual oscillations by means of a glass disk which we slide back and forth on the celluloid and which has lines ruled on it 0.1 mm. apart. The measurements are made under an erecting microscope at a magnification of 15 diameters.

If the assumption is admitted that a muscle will stretch a certain distance in response to a given weight and that this distance will vary for that particular weight inversely as any variation in resistance of the muscle, then it follows that for any value of T_o, the size of the angle swept out by the arm in passing from the neutral or starting point to the point where T becomes equal to T_o is inversely proportional to the tonic state of the muscle or the capability of the muscle to resist the pull of the driving weight. Now, it is known that muscles do not behave exactly in accord with the laws of truly elastic bodies, but exhibit a certain amount of plasticity. However, in order to obtain a comparative measure of tonus from person to person we are at this point arbitrarily assuming that muscle tissue is elastic up to the point where the muscle resistance becomes equal to the pull of the driving weight. We shall present some evidence from other methods of measuring muscle tonus to show that within the limits of the present method of measurement, this assumption is reasonably well based on experimental facts, but we will not enter further into a discussion of the matter at this time.

CASES ILLUSTRATING THE METHOD OF MEASUREMENT

Figure 3 is a reproduction of the curves obtained at one sitting from the right arm of a normal woman, aged 35, a laboratory technician of rather slender build, weight, 120 pounds (54.4 Kg.), and height, 5 feet 5 inches (165.1 cm.). She was loose jointed, so that with the elbow in complete extension, instead of making an angle of 180 degrees with the upper arm, the forearm made an angle

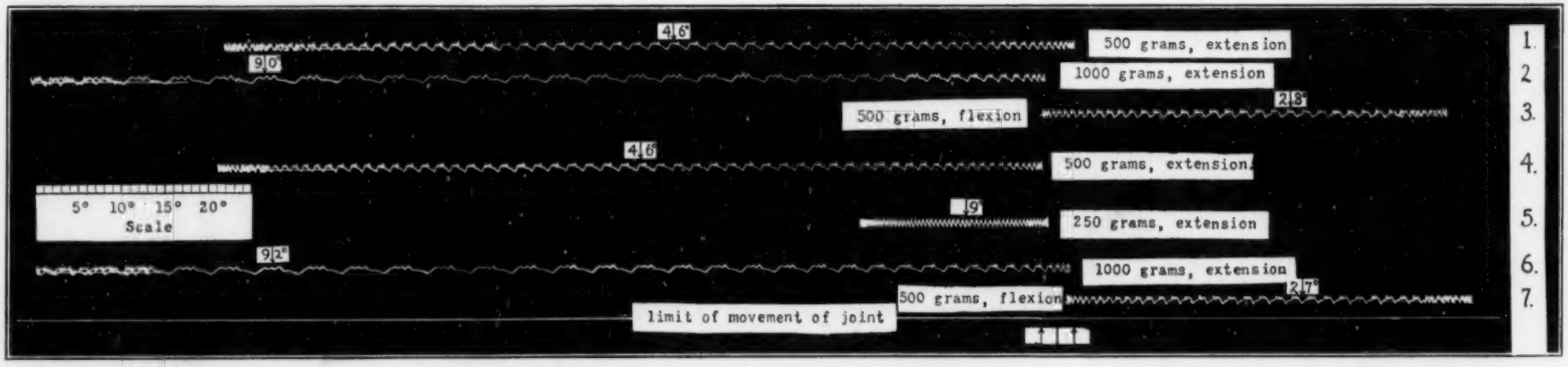

Fig. 3.—Tracings obtained at one sitting from a normal woman.

of 195 degrees. Incidentally, this subject gave tonus readings which showed that her muscles had the least amount of tonus found in any normal adult that we had studied up to this time. Her tonus readings had been less variable from time to time than is the case with most of our subjects. Hence it was evident that her ability to relax was extraordinarily good.

In order to obtain the tracings in figure 3, the method was followed as already described. The topmost tracing was taken first, and the others followed in order from above downward. The weight used for each curve and the direction of the curve are indicated at the beginning of each individual tracing. The two arrows at the bottom of the figure pointing upward represent the angular range of the neutral point within which the forearm comes to rest after being allowed to swing freely in flexion and extension without the application of forces other than those in the muscles of the arm. We have already indicated that the forearm normally approximates closely an angle of 90 degrees with the arm at this point. In this particular case, the neutral point varies within a range of 4 degrees (the angular distance between the arrows). In the case of a movement of the elbow joint in extension, the curves are to be read from right to left, and in the case of a movement in flexion, they are to be read from left to right. The first tracing was obtained by placing a 500 Gm. weight on the machine as the motive force and the forearm was moved by this weight into extension. The longest oscillation of the time marker is indicated in the figure by an arrow directed downward and it will be seen that this longest oscillation is situated 46 degrees from the beginning of the tracing. Since a 500 Gm. weight was used to produce this tracing, one can say that at 46 degrees the resultant of the tonus torques of the muscles around the elbow is equal to the machine torque of $500 \times 980 \times 10.42 - 71.500 = 5{,}034{,}300$ in this particular tracing.

We shall compare this result with tracing 2 in figure 4, which is from the right arm of a normal man, aged 36, weight, 155 pounds (70.3 Kg.), and height, 5 feet 8 inches (172.7 cm.). This man has given readings with little variation from time to time and his tonus torque is near the average for the men whose readings we have obtained. In this case 500 Gm. constitutes the motive force so that the driving torque is likewise 5,034,300. It will be seen that at 16 degrees the resultant of the tonus torques of the muscles around the elbow is equal to this driving torque.

Since, for a given machine torque (in this case 5,034,300) the state of muscle tonus is inversely proportional to the angle at which the tonus torque becomes equal to the machine torque, one can immediately say for these two tracings that the state of tonus of the man (T_m) is to that of the woman (T_w) as 1/16 is to 1/46, or

$$T_m : T_w :: 1/16 : 1/46$$

$$\frac{T_m}{46} = \frac{T_w}{16}$$

$$T_m = \frac{46\, T_w}{16}$$

$$T_m = 2.88\, T_w.$$

In other words, with a machine torque of 5,034,300 the torque of the muscle tonus of the man is 2.88 times as great as that of the woman, on the basis of comparison of these two particular tracings.

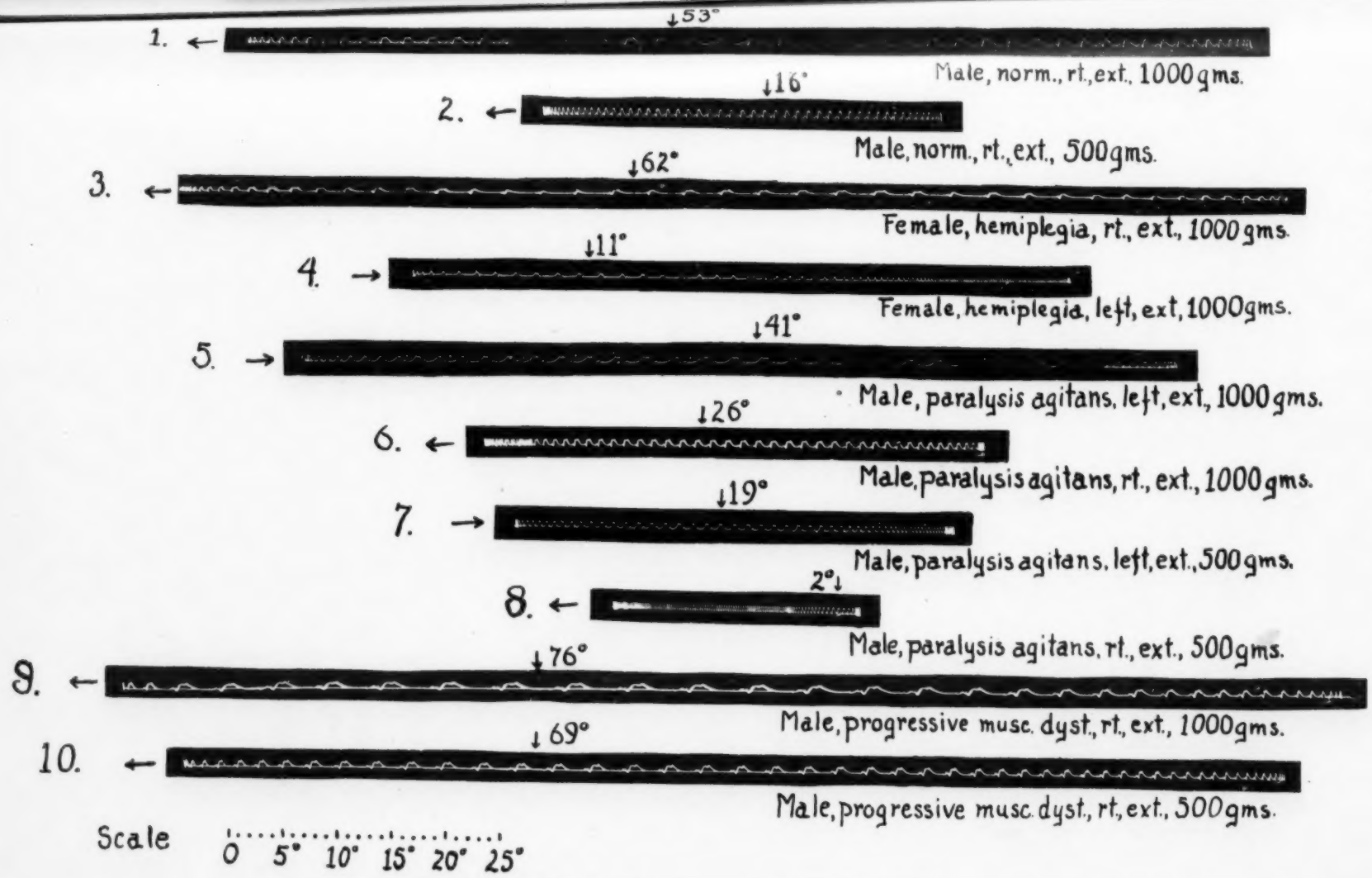

Fig. 4.—Selected tracings from normal and pathologic cases.

The foregoing remarks illustrate the method by which we are able to make comparative measurements of muscle tonus from subject to subject. It is now necessary to refer to factors of error in the method:

The matter of frictional variation in the machine has already been discussed. The error here is evidently of no troublesome significance.

The location of the point where T equals T_o introduces an error into the evaluation. It is evident that the longest oscillation does not locate a point but rather a line which is as long as the oscillation. Hence, when we say that at 46 degrees the tonus torque equals the machine torque, we really mean that 46 degrees is the middle point of the longest oscillation and that the point at which the two torques are equal lies somewhere within the angular distance swept out during this fastest fiftieth of a second. This error is less than $\pm$ 3 per cent. If the location of the point is at or nearly at the end of an oscillation, the next neighboring oscillation will have a length practically identical with that of the first one. In this case the point is located with greater accuracy. Since our readings show that muscle tonus varies a good deal more than this error in normal subjects from time to time or even during the same sitting, and since tonus varies throughout a range of several hundred per cent in comparing normal people with one another, the error, though not desirable, is not serious.

In connection with the location of the point where T equals T_o, a word of caution is necessary in order to avoid a rather obvious possibility of error. In tracings 2 and 6 of figure 3, it is seen that the 1,000 Gm. weight produces a machine torque which is greater than the muscle torque all the way out to a 90 degree or 92 degree angle. Now, in most cases (and this one happens to be an exception as already noted), the elbow is completely extended at an angle of 90 degrees from the start of the curve. This means that if the tracing extends more than 90 degrees in the ordinary individual, the over extension is probably due to factors other than simple extension of the elbow joint; either the weight is so large that it has pulled the arm rest out from under the forearm of the patient, or, as is more commonly the case, the shoulder has been displaced forward. Consequently, it is unsafe to accept a reading of the point where T equals T_o if that point is more than about 85 degrees from the start of the tracing. The limits in flexion are still more circumscribed. We have frequently recorded the mechanical limits of motion of the joint graphically by placing the time marker tip against the tracing and by flexing and extending the patient's elbow by hand as far as it will go in either direction. (Compare with the line marked "limit of movement of joint" in figure 3.) However, there is a tendency on the part of the examiner to use too much force and thus artificially to extend these limits considerably further than would be the case ordinarily.

We have frequently referred in this paper to the resultant of the torques of the individual muscles around the elbow joint. Figure 5 will serve to illustrate the full significance of this phrase. Only the biceps, brachialis and triceps are considered in this figure. The other flexors like the brachioradialis, and the other extensor (anconeus) are not considered, as they simply complicate the picture without adding anything to the clarity of the concept with which we are dealing. The circular line in the figure represents the circumference of the main fiber disk of the apparatus which is rotated along with the elbow joint. The figure is drawn to scale with a skeleton in the anatomic collection. The forearm is in the position of the start of a tracing at a 90 degree

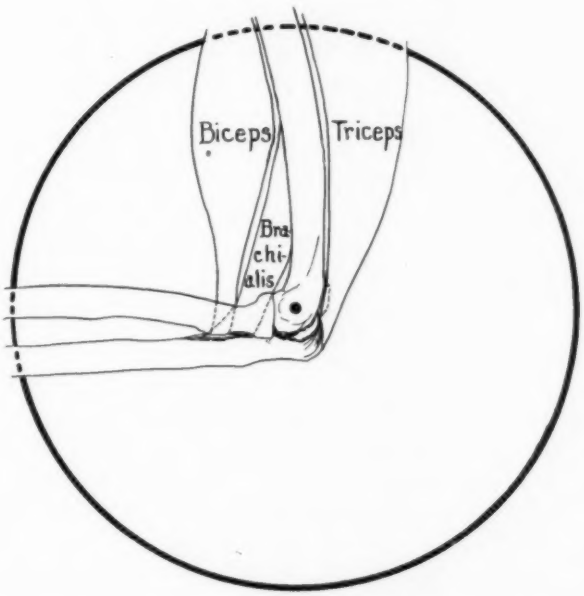

Fig. 5.—Diagram of certain muscle relationships around the elbow.

angle with the arm. It is evident that at this neutral point or point of equilibrium the flexors and extensors may be and probably are exerting some pull against one another. If the disk is rotated in a counter-clockwise direction by the driving weight the triceps will tend to be more and more relaxed and the biceps and brachialis will be more and more stretched. Obviously it can only be the difference of these antagonistic pulls which we are able to measure in terms of torque.

Individual anatomic variation in the length of the perpendicular distance from the axis of rotation of the joint to the line of force of the muscles around the joint most certainly occurs. Variation in this distance between two subjects results in a variation of muscle torque

even though the tonus of the muscles might be in each case the same. In more simple language, the leverage of muscles on the joint may vary without any difference in the force of muscle pull. Very few applicable data are available on this point, though Braune and Fischer [9] and Fick [10] have studied the elbow torques in six cadavers in considerable detail. Their observations are evidently insufficient for statistical deductions but are sufficient to demonstrate to any worker in the field of human muscle tonus measurement that if he desires to analyze tonus further than the resultant tonus torque of the muscles playing around a given joint, he is confronted by a large amount of labor on the physics of muscle and joint relationships. The reader will note that we are measuring only this resultant torque of the individual muscles around the elbow and are not at this time concerned with the finer analysis of individual muscle tone. On the other hand, it is already evident to us that the physiologic variations in muscle resistance are so wide as to exclude the likelihood of their being explicable solely on the basis of anatomic variation. This is particularly apparent when we see the amount of variation in muscle tonus torque in a single individual even during the time of a single sitting, and even after considerable experience over months of time as the subject of experimentation.

COMMENT

We have already mentioned the observation that when the elbow joint is freed from the effect of gravity and is allowed to rotate according to the pull of the relaxed muscles in the arm, the forearm constantly assumes, in normal people, an angle of 90 degrees (plus or minus 2 or 3 degrees) with the arm. This fact is indeed interesting when we consider it in connection with the hypothesis of postural tone as proposed by Sherrington [11] and much discussed by his followers in the physiologic field. Sherrington, if we understand him correctly, has assumed "reflex tonus" to be present in those muscles which tend to support the body (or a part of the body) in normal posture, and absent or nearly absent in the antagonists of these muscles. Now it is evident that there is no correspondence of this neutral point with the arm on our apparatus with any normal erect posture of the human being unless it be the fetal position. It is also evident that there is tonus in both extensors and flexors when we take our tracings and locate

9. Braune, W., and Fischer, O.: Die Rotationsmomente der Beugemuskeln am Ellbogengelenk des Menschen, Abhandl. d. kön. sächs. Ges. d. Wissensch. 26, math. phys. Classe **15**:243, 1890.

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the point where T equals T_o. Further, the neutral point commonly remains at 90 degrees in cases of hemiplegia and paralysis agitans and during surgical anesthesia. In one case of progressive muscular dystrophy which we have studied, the arm (not forearm) flexors of the elbow were almost completely atrophied and paralyzed whereas fair strength remained in the extensors. In this case the neutral point was displaced outward from 14 degrees to 20 degrees from the normal 90 degree angle. The hemiplegic, parkinsonian and anesthetized subjects show that a change of tonus does not necessarily result in a change of the neutral point provided the whole musculature of the joint is affected by the same pathologic influence. The case of dystrophy shows that when the flexor or extensor tonus is involved without involvement of the antagonistic muscles, the neutral point is immediately changed in correspondence with the decreased force from the paralyzed and atrophied muscles. In our opinion, these observations constitute strong evidence against the theory of postural tone. The flexor attitude of the arm in hemiplegia is most likely due to the fact that as the tonus in the flexors and extensors increases, there is a tendency for the forearm to approximate the 90 degree angle, and the stronger the tonus the more the flexors can overcome the effect of gravity which normally tends to keep the arm in the extended position. In regard to many questions that arise from a study of this sort, it is evident that only meager conclusions are permissible at the present stage of the work and that a statistical handling of the data under collection will be necessary before the formulation of a statement of the normal range of tonus in quantitative terms and before accurate comparisons can be undertaken. This work is in progress, and will constitute the material for future communications. On the other hand, certain of our observations are evident enough at the present time so that we are justified in presenting them here without much fear of later contradiction.

In figure 3, if one compares tracings 1 and 4 with tracings 3 and 7, one might be led to think that there is nearly twice as much resistance in the movement of flexion as there is in passive extension, or in other words, the extensors are nearly twice as resistant to passive flexion as the flexors are to passive extension with the same driving torque. However, it is to be remembered that one is comparing torques, and it is apparent from figure 5 that the leverage of the triceps is certainly much less than that of the biceps and brachialis. This leverage is just the reverse of what one would expect from the tonus torque readings, as such a relationship would tend to make the tonus torque less rather than more. It can only be that there is intrinsically more pull in the triceps at a certain angle of flexion than there is in the flexors in passive extension. Attention is called to this matter only to show that in the present state of information, it is not safe to compare any tonus torques

other than those that are identical in the joint selected and in the particular action of the joint that is under consideration. It is, however, permissible to compare homologous movements of homologous joints; for instance, it is correct to compare the tonic state of the flexors of the right elbow with the flexors of the left; it is also correct to compare the extensors of one person with those of another in the same joint.

TABLE 1.—*Data on Muscle Tonus of a Normal Woman*

Five hundred Gm. driving the machine.	Machine torque = 500 × 980 × 10.42 = 5,034,300			
	Right Elbow		Left Elbow	
Date	Degree of Extension	Degree of Flexion	Degree of Extension	Degree of Flexion
6/ 9/27	26	25	49	25
	48	26	57	23
	46	..	..	..
6/14/27	43	26	52	21
	46	..	57	22
6/23/27	41	26	63	26
	42	26	56	26
	42	..	..	..
6/24/27	40	25	51	26
	44	25	59	27
7/ 6/27	49	24	56	25
	46	23	50	25
9/16/27	50	23	53	24
9/17/27	..	..	58	25
9/19/27	..	..	54	23
9/21/27	..	..	54	23
9/26/27	46	23	52	26
	46	27	48	26
10/ 1/27	..	..	48	22
	..	..	52	23
	..	..	49	..
Average	43.6	24.9	53.6	24.3

TABLE 2.—*Data on Muscle Tonus of Medical Student*

Five hundred Gm. driving the machine.	Machine torque = 500 × 980 × 10.42 = 5,034,300			
	Right Elbow		Left Elbow	
Date	Degree of Extension	Degree of Flexion	Degree of Extension	Degree of Flexion
6/24/27	34	24	85	46
	29	25	18	39
	26	24	..	39
	28	22	..	..
7/14/27	42	28	26	40
	42	42	..	35
9/16/27	18	32	25	33
	37	..	20	..
Average	32.0	28.1	34.8	38.7

We have already indicated that considerable variation in the tonus values occurs with the same individual from time to time, and the amount of this variation is different from person to person. Tables 1 and 2 illustrate this statement with specific values for tonicity. The reader is reminded that the magnitude of the figures in degrees is inversely proportional to the tonic muscular resistance. In table 1 are collected the readings of the tracings with a 500 Gm. weight driving

the apparatus for the two elbows in flexion or extension on the dates indicated. The data in table 1 have all been obtained from the same person from whom figure 3 was obtained.

It is seen from table 1 that, with the single exception of the reading of 26 degrees at the top of the first column, the values for this subject have no more deviation from the average than is usual with most mensurable biologic phenomena.

In table 2, which is taken from a male medical student, aged 27, height, 5 feet 10 inches (177.8 cm.), weight, 142 pounds (64.4 Kg.), an entirely different type of individual from the standpoint of tonus is represented. Several large deviations from the average reading are apparent on inspection of this table. It might be argued that these deviations are due to lack of cooperation on the part of the subject, to the inconstancy of the machine, or to careless technic in placing the forearm on the machine. Now this man has been a research assistant in this laboratory in connection with animal experimentation for over a year, and we have always found him intelligent, interested in our research problems, and cooperative to the extent of frequently doing work beyond his recompense merely to insure the success of an experiment. If the machine were not working properly, or our technic were so inaccurate as to account for these deviations we would not expect constancy of results with a person like the one from whom table 1 is obtained. These two tables illustrate two common types of people, as nearly as we can judge, as regards their ability to give constant tonus readings. The physiologic significance of these types is not clear to us as yet, but it has interested us to find that the persons with marked variation of tonus values are commonly nervously hyperexcitable. There are found, of course, persons representing all stages between these two types. It is obvious that this irregularity in certain persons complicates the study of normal and pathologic muscle tonus, and that final conclusions must be postponed until sufficient data are available for a suitable statistical study.

Now that we have brought out the constancy of the tonus readings of the case from which table 1 is constructed, we are in position to describe an experiment with this person which confirms a statement expressed mathematically in the foregoing test. The reader will recall that mathematically we rid ourselves of the moments of inertia of the arm and machine to obtain our equation "$T = T_o$" by selecting the point where acceleration is absent in the curve. In order to test the validity of the formula we proceeded as follows:

We used, as the subject of experiment, the person just mentioned because of the lack of large variation in her tonus readings. We took curves in extension of her arm alone on the machine, using a 500 Gm. weight as the motive power. Before and after the taking of certain of these tracings we placed a 200 Gm.

weight on the arm rest along with the patient's arm and at a distance of 40 cm. from the axis of rotation thus giving us an additional moment of inertia of $200 \times 40^2 = 320{,}000$ and took a tracing in extension. Then we replaced the 200 Gm. weight with one of 750 Gm. at 40 cm. from the axis, giving an additional moment of inertia of $750 \times 40^2 = 1{,}200{,}000$ and took another tracing. The results are shown in figure 6. In tracings 3, 4, 5, 6 and 9 one sees that the point where T equals T_0 is located 48, 49, 48, 51 and 49 degrees respectively, from the start of the tracing. Thus the fluctuation in tonicity with additional moments of inertia added to the machine are no greater than the fluctuations observed in this person with her arm alone on the machine. Since the moment of inertia of the arm of this patient was calculated to be approximately 700,000; and since in the one case we added a moment of inertia of 320,000 and in the other 1,200,000, it is evident experimentally that fluctuations in arm size do not influence this method of finding a value for the resultant of tonus torques about a joint in the living person.

Inspection of figure 6 shows that on the average the arm travels faster when it is alone on the machine (tracings 3, 6 and 9) than when a moment of inertia of 320,000 is added (tracing 4). Also tracing 4 shows greater speed than in the case of tracing 5 where the additional moment of inertia is 1,200,000. This is as one would expect logically. Since, however, tonus is probably a reflex mechanism, variation in the speed of application of the machine torque might just as logically cause variation in tonicity. That demonstrable variations in tonicity do not occur within the limits of variation in speed caused by differences in moments of inertia of various arms is evident from this figure.

Figure 4 shows how this method of measuring muscle tonus torque brings out the changes in pathologic cases. The tracings are all extension curves. It is to be remembered that in making comparisons the machine torques should be the same. Therefore, curves obtained with the 1,000 Gm. weight are not to be compared with those obtained with the 500 Gm. weight. Tracings 1 and 2 are from a normal man as already mentioned. Tracing 3 is from the normal side of a hemiplegic woman. The tonus in this arm is slightly less than that in tracing 1. On the hemiplegic side (tracing 4) the tonus torque is nearly six times as great as in tracing 3 and five times as great as in tracing 1. Tracings 5 to 8 are from a case of paralysis agitans which exhibits a bilateral tremor, more marked on the right, and evidently, from ordinary observation, more rigidity on the right. Tracing 5 from the left side, shows slightly more tonicity than tracing 1. Tracing 7 shows slightly less rigidity than tracing 2. Hence there is little evidence of increased tonus on the left as compared with the normal. A similar comparison of the right arm (tracings 6 and 8) shows far more resistance than in the two normal curves. The slow curves in paralysis agitans (tracing 8, for example) show an irregular speed of passive movement apparently due to the tremor so these slow curves are of somewhat uncertain significance quantitatively. Tracings 9 and 10 show

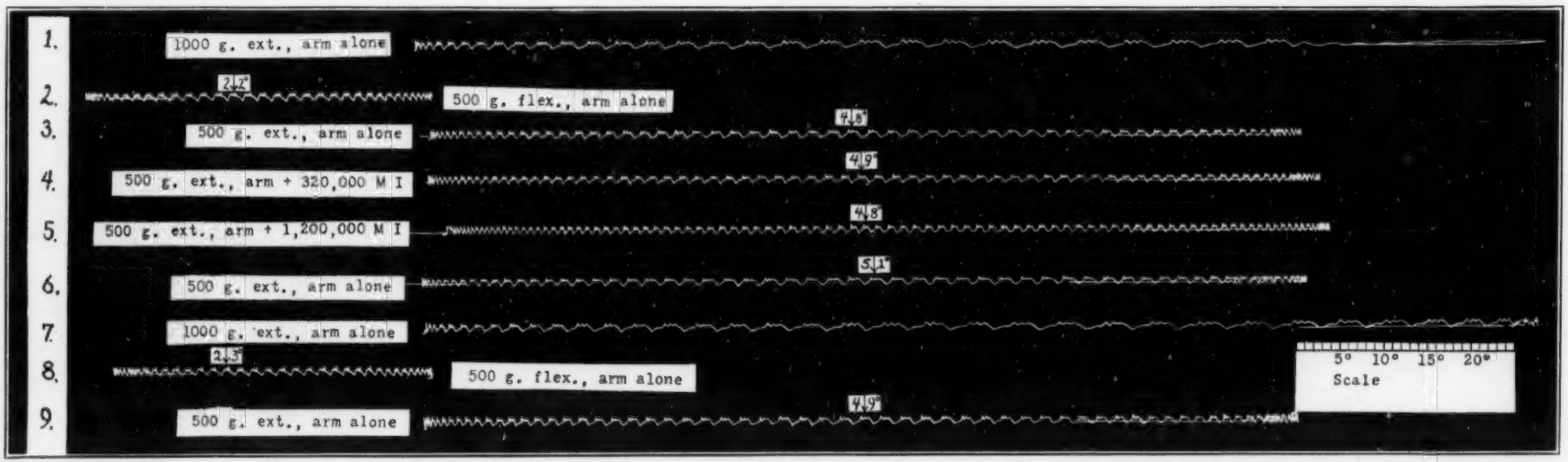

Fig. 6.—Tracings to demonstrate that the moments of inertia have no effect on the evaluation of tonus torques.

two fast curves with high angular values of the reading points indicating marked tonus reduction in a case of progressive muscular dystrophy.

SUMMARY AND CONCLUSIONS

1. A method of measuring human muscle tonus torques is presented which is unique in that: (*a*) it is rapid and accurate enough to catch tonus changes of short duration and small size; (*b*) the error is probably not over ± 3 per cent; (*c*) the variations in the moment of inertia and weight of the extremity under study do not influence the evaluation of the tonus torque; (*d*) the personal equation of the observer is practically eliminated because the tracings are obtained automatically and cannot be analyzed readily until after the termination of the examination.

2. The natural position of the human elbow, from the standpoint of equilibrium of tonic forces acting on it in extension and flexion, is with the forearm at a 90 degree angle from the arm. Diffuse changes in tonus involving flexors and extensors alike (for instance parkinsonism and hemiplegia) commonly do not alter this neutral position. Paralysis of the extensors or of the flexors causes the elbow to assume a new neutral point depending on the action of the paralyzed muscle group and the presence of tonus in the antagonistic group. The flexor position of the elbow in hemiplegia is no doubt determined by this balance of forces and by the fact that the tonus torques become large enough to overcome the force of gravity on the forearm.

3. Tonus is present in both the flexor and extensor muscles of the elbow. The theory that tonus is present in the "postural muscles" and probably absent or nearly absent in the antagonists is probably fallacious.

4. Normal persons may be classified broadly into two different types as regards their muscle tonus, though evidently there are many examples falling between the two classes. The more phlegmatic persons give little variation in the values of their tonus torques from time to time. Excitable persons show marked fluctuations of their tonus torques.

5. Changes in tonus in hemiplegia, paralysis agitans and progressive muscular dystrophy are illustrated; the machine readings confirm the usual clinical observations relating to increase and decrease of tonus, as the case may be, in these conditions.

CEREBRAL CIRCULATION

III. THE VASOMOTOR CONTROL OF CEREBRAL VESSELS *

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AND
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It is a common opinion among physiologists that the cerebral blood vessels do not possess effective vasomotor nerve control. The strongest evidence on which this opinion rests has been brought forward by a number of English physiologists, notably Roy and Sherrington,[1] Bayliss and Leonard Hill,[2] Hill and Macleod [3] and Florey.[4] Several Germans [5] have also contributed evidence pointing toward the same conclusion. On the other hand, many important observations which are difficult to reconcile with this point of view have been reported by investigators from Germany,[6] from France,[7] from the United States,[8] and elsewhere.[9] Our own experiments bring new evidence in favor of the functional activity of vasomotor fibers in the blood vessels of the pia mater.

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* From the Department of Neuropathology, Harvard Medical School.

* A preliminary report of this work has been published in the Proceedings of the American Society for Clinical Investigation, in a paper read in May, 1927.

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The question whether or not the cerebral vessels possess a vasomotor control is not merely of academic interest, for many clinical conditions such as convulsions, migraine and transient cerebrovascular accidents have been interpreted as cerebral vasomotor phenomena. Examples of clinical observations bearing on this are the sudden blanching of the cerebral cortex of a patient on the operating table at the onset of an epileptic seizure,[10] and the blanching of the retina or "spasm" of retinal arteries during epileptic convulsions,[11] during attacks of migraine and during transient amblyopia. Naturally, however, it has been difficult under clinical conditions to control such variables as blood pressure and intracranial (or intra-ocular) pressure at the same moment that the "blanching" or "vascular spasm" takes place. And yet without such controls one cannot say whether or not the observed vascular changes are true vasomotor phenomena. Other possibilities must be considered: sudden emptying of the vessels or passive collapse of their walls following a rapid drop in blood pressure, or constriction resulting from direct stimulation of the vessel walls by physical or chemical alterations in composition of the blood. These and perhaps other variables must be understood before a correct interpretation can be reached.

A search of the literature on the experimental work discloses a striking conflict of opinion, and this conflict seems to have been due to several distinct causes. Of these, three stand out conspicuously:

1. The technic employed in the past for direct inspection of pial blood vessels has been, with few exceptions, too crude to permit the measurement or even the detection of slight changes in vessel caliber, i. e., changes of the magnitude of those which we observed after sympathetic stimulation.

2. Investigators[12] have often failed to take full account of the fact that large fluctuations in systemic blood pressure may obscure slighter vasomotor changes in caliber of the cerebral vessels. This was emphasized by Wiggers[13] more than twenty years ago. In our experiments, e. g., after intravenous injection of epinephrine, rises in arterial pressure of large extent were sufficient to overcome the comparatively weak tendency of the pial arteries to constrict, and this constriction became evident only when the arterial pressure began to fall.

3. The use of indirect methods alone is hazardous. Inferences concerning changes in caliber of the cerebral arteries, drawn even

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12. Footnote 5, third and fourth references.

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from careful simultaneous measurements of alterations in intracranial and extracranial vascular pressures, cerebrospinal fluid pressure and brain volume, have led to unwarranted conclusions, for there still remain other variable factors not taken into account. An example of logical reasoning leading to an erroneous conclusion through the use of incomplete data is seen in the study of asphyxia. This condition, with its associated rise in systemic arterial pressure, has been used [2] to demonstrate that cerebral arteries dilate merely in a passive way, owing to the increased hydrostatic pressure within their walls. From indirect methods of examination alone this seems to be the most reasonable explanation for the increase in caliber. If one uses direct measurements of the vessels' diameters, however, combined with the indirect method of determinations of pressure, it is found that in asphyxia the dilatation of the cerebral (pial) arteries precedes the rise in systemic arterial pressure. Though this rise in pressure may and probably often does still further increase the dilatation of the arteries, the dilatation may rapidly increase while the arterial pressure steadily falls.[14]

The need for measuring simultaneous variations of intracranial pressure and of the pressure within the blood vessels inside and outside the skull was not recognized until the advent of the English physiologists just mentioned. This recognition and the work prompted by it was a big step in advance, but until still further data were obtained, derived from actual measurements of changes in caliber of the blood vessels themselves, the method remained an indirect one, insecure because not all of the factors were known and inference alone could not safely bridge the gap.

The aim of the work here presented has been to secure evidence of a convincing nature concerning the presence of vasomotor control of cerebral vessels by combining the two methods indicated—that is, the measurement of pressures, and the simultaneous measurement of vessel diameters by direct observation.

METHOD

The method employed has already been described in detail.[15]

Cats were anesthetized with 1 per cent iso-amyl-ethyl barbituric acid [16] (from 7 to 9 cc. per kilogram of body weight, injected intraperitoneally) in all except two cases, when ether was used. Throughout all the experiments the animal lay on its right side in the horizontal position, and the body temperature was maintained by electric bulbs beneath the animal board and a blanket above the

14. Forbes, H. S., and Wolff, H. G., to be published.

15. Forbes, H. S.: The Cerebral Circulation: I. Observations and Measurements of Pial Vessels, Arch. Neurol. & Psychiat. **19**:751 (May) 1928.

16. Eli Lilly & Co., of Indianapolis, furnished us with this material for experimental purposes.

animal. The skull, held rigidly by a strong clamp, was trephined, and a specially devised glass window with an externally threaded steel rim was screwed into the trephine opening after bloodless removal of a piece of dura. Then cerebrospinal fluid, previously removed by needle from the cisterna magna, was injected beneath the window through a hole in the rim. The air trapped beneath the glass was thus replaced by a normal fluid (often Ringer's solution was used) and the air allowed to escape by a second hole in the window's rim. Both holes were then sealed, and the skull became as nearly a rigid box as it is normally. A microscope on an adjustable stage was now clamped to the table, above and perpendicular to the plane of the skull window, and an oblique beam of light, filtered through a green solution to bring the vessels into sharp relief, was focussed on the window and the brain surface beneath (fig. 1). A micrometer scale in the ocular of the microscope could be rotated across any pial artery or vein which it was desired to measure. The finest division of the ocular scale, by calibration with an object micrometer scale, was found to be equivalent to 9 microns. Our accuracy in measuring vessels did not greatly exceed this, as a rule, though in clear fields a change in vessel diameter of 4.5 microns could be distinguished with some precision. Our custom has been to record changes in arterial diameter of less than 9 microns by a plus or a minus sign. The limit of precise measurement, therefore, is 9 microns, and a change of 4.5 microns (which amounts to 4 per cent of the smallest vessels measured and 1.2 per cent of the largest) must be considered doubtful. The degree of accuracy of measurement was checked by several observers, and during the experiment it was customary occasionally to change observers. For photography, the ocular of the microscope was replaced by a small camera which was equipped with a sliding magazine for exposures at intervals of about ten seconds (fig. 1).

Cerebrospinal fluid pressure was registered by a glass manometer of 1 mm. bore, filled to a point 100 mm. above the cistern with Ringer's solution and connected by a rubber tube and a needle with the cisterna magna. Systemic arterial pressure was measured by a mercury manometer and a cannula in the femoral artery. In one experiment, pressures in the peripheral and the central ends of the severed carotids were recorded, and in another experiment pressure in the saggittal sinus was recorded by Weed's[17] technic. In all the experiments, at intervals of one minute or less, observations were made of the various pressures and of the diameter of the pial artery under observation. Graphic charts have been made from the data thus obtained.

In the experiments in nerve stimulation, the vagi in the neck were exposed on both sides and the cervical sympathetic nerve was separated from the vagus (with its included depressor fibers). Ligatures were placed about both nerve trunks on either side of the neck, but were usually not tied until the skull window was in place and observations had been started. Then the four ligatures were tied, and bipolar silver chloride electrodes placed on the nerve to be stimulated above the ligature, that is, toward the head of the animal. In many cases the nerve trunks were cut below the ligatures to insure against transmission of nerve impulses back along the fibers of the central end of the sympathetic. For the most part, a faradic current was used. It was taken from an inductorium[18] (coil distance from 2 to 13 cm.) supplied by one dry cell

17. Weed, L. H., and Hughson, W.: Am. J. Physiol. **58**:101, 1921.

18. The inductorium was of the usual type, containing an iron core, and was made by the Harvard Apparatus Company.

without external resistance, giving a current in the primary circuit of about 1 ampere. The frequency of interruption in the primary circuit was reduced to 28 per second by lengthening and weighting the vibrator. In some experiments we used a galvanic current from one dry cell, interrupted so as to give stimuli of from one fiftieth to one hundredth of a second in duration, at rates of approximately forty, fifty and sixty per second. This type of stimulation seemed not to be any more effective than the faradic stimulation and later was abandoned. The dilatation of the pupil and the retraction of the nictitating membrane were occasionally examined as evidence that the cervical sympathetic nerve had actually been stimulated, although it was noted that the eye and the pial artery did not always have the same threshold.

In the experiments in which epinephrine was employed, freshly made solutions[19] were used in dilutions of 1:10,000, 1:100,000 and 1:500,000 in Ringer's

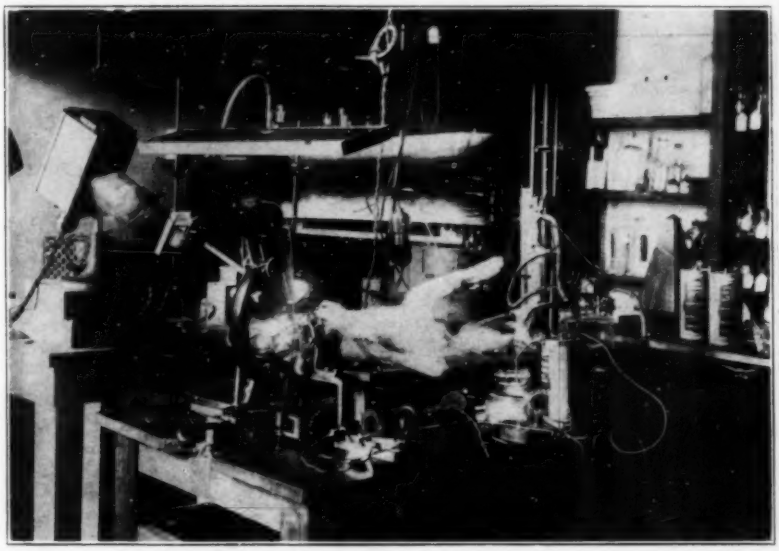


Fig. 1.—Apparatus showing a cat with its head firmly fixed in the specially devised head holder; the microscope mounted on a mechanical stage permitting of fine adjustments and movements; the camera with a magazine containing five plates and permitting exposures in rapid succession mounted on the microscope; Point-o-lite lamp with light beam directed on the surface of the brain and color screen, and cannula in the femoral artery with the usual apparatus for recording blood pressure.

solution. The drug was injected into the saphenous vein or into the carotid artery, or it was applied locally to the vessels under observation by irrigation beneath the skull window. When the latter method was used, a preliminary

19. Adrenalin chloride tablets from Parke, Davis and Company were used in preference to the adrenalin chloride solution (Parke, Davis and Company), for the latter has a slightly acid reaction and contains chloretone as a preservative. Gruber, C. M., and Roberts, S. J.: J. Pharmacol. & Exper. Therap. **27**:335, 1926.

irrigation with Ringer's solution of approximately the same temperature was twice used as a control, and an appreciable change in the diameter of the pial arteries was not observed.

EXPERIMENTAL RESULTS

Sympathetic stimulation was followed by constriction of arteries in the pia (the arteries varying from 110 to 340 microns in diameter) in each of the twenty-three animals examined, and in 88 per cent of the total number of stimulations (figs. 3 and 4 and table 1). In the seventy-five trials in which there was no fall of blood pressure, constriction of at least sufficient extent to be accurately measured (9 microns or more) was noted in 87 per cent, and constriction of 13.5 microns or more, in

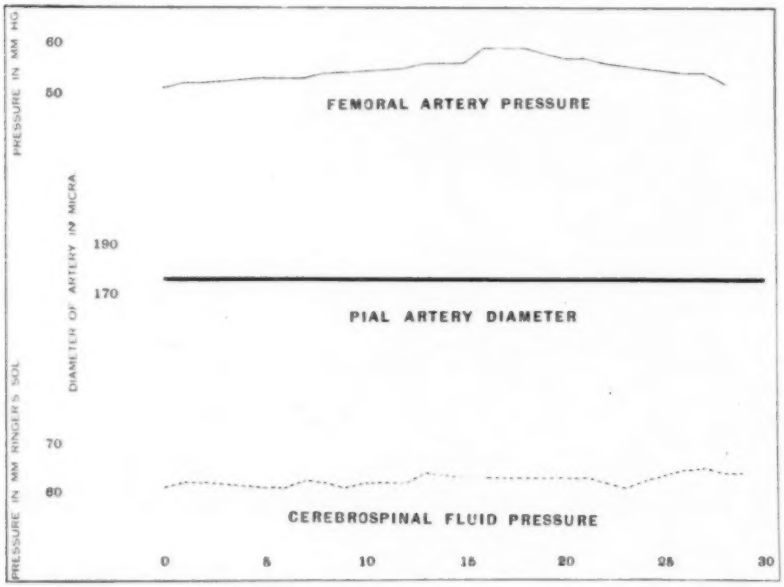


Fig. 2 (experiment 68).—Control. Demonstration of the constancy (absence of spontaneous fluctuations) in the diameter of the pial artery during a thirty minute control period. There was no change in the diameter of the pial artery and only slight variations in the pressure in the femoral artery and in the cerebrospinal fluid pressure. In this figure and figures 3, 4, 6, 7, 8, 11, 14, 15 and 16, the ordinates represent millimeters of mercury, microns and millimeters of Ringer's solution, respectively. Observations of pressures and measurements of diameters of the pial artery were made at one minute intervals or less in all instances. Cross hatched areas or arrows at the bottom of the charts indicate periods of stimulation or injection. The broad line indicating diameter of the pial artery is so plotted that its upper edge records the correct measurements and time relations. The thin line indicates femoral artery diameter, the heavy line, pial artery diameter and the broken line, cerebrospinal fluid pressure.

60 per cent. Figure 5 shows a characteristic constriction after sympathetic stimulation. The degree of constriction in the group varied from 3 to 18 per cent of the artery's initial diameter. In no instance was complete obliteration of the lumen seen in the arteries or in the smallest arterioles, but it may be that the character and the depth of anesthesia exerted an important influence on the degree of vascular response to stimulation. When the animal was under the influence of ether the responses of the pial arteries were similar in kind to those observed

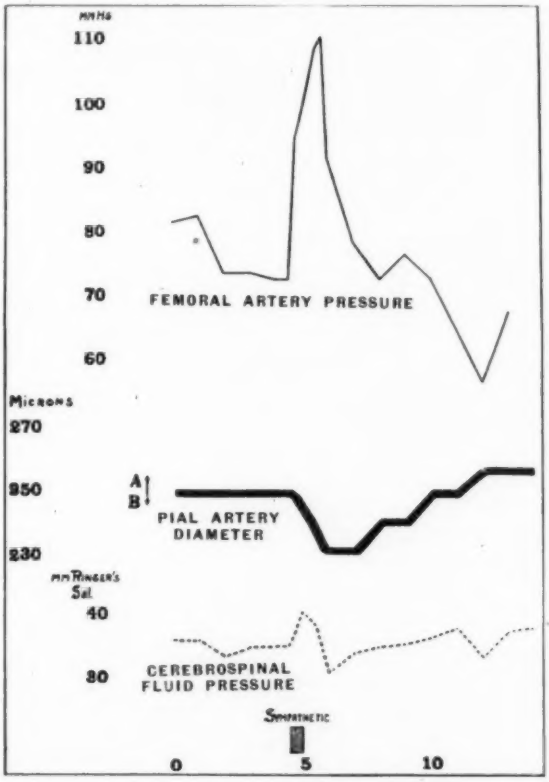


Fig. 3 (experiment 21).—Sympathetic stimulation. After a control period of four and a half minutes, a half minute faradic stimulation (coil distance 10 cm.) of the left cervical sympathetic nerve was started. It was followed by an immediate rise in arterial pressure of 38 mm. of mercury and, after a delay of a fourth of a minute, a constriction of the pial artery amounting to 7.1 per cent of its initial size. The constriction occurred during the rise of arterial pressure. The artery slowly returned to its previous caliber and then dilated slightly while the arterial pressure fell rapidly to a point below its previous level. The cerebrospinal fluid pressure at first rose with the sharp rise in arterial pressure, and then fell during the constriction of the pial artery and slowly regained its level as the artery dilated. In this figure and in figures 4, 6, 7, 8, 11, 14, 15 and 16, the arrow *AB* (9 microns) represents a change in arterial diameter which could be accurately measured. A change of half this extent is of doubtful validity.

when iso-amyl-ethyl-barbituric acid was used. In unanesthetized animals and in man, data regarding the degree of constriction or dilatation of the cerebral arteries are not available. The latent period between the beginning of stimulation and the beginning of recognizable change in the diameter of the artery was usually from fifteen to thirty seconds. In a smaller group of fourteen trials, in which the blood pressure showed a decided fall, an overflow of the stimulating current to the vagus was suspected twice. In one of these instances, dilatation alone occurred; in the other, constriction followed by dilatation. The rest of the trials in this group (except two in which change did not occur) resulted in constrictions. Since these constrictions might have been due to passive collapse of the arterial wall following the sudden fall in blood pressure,

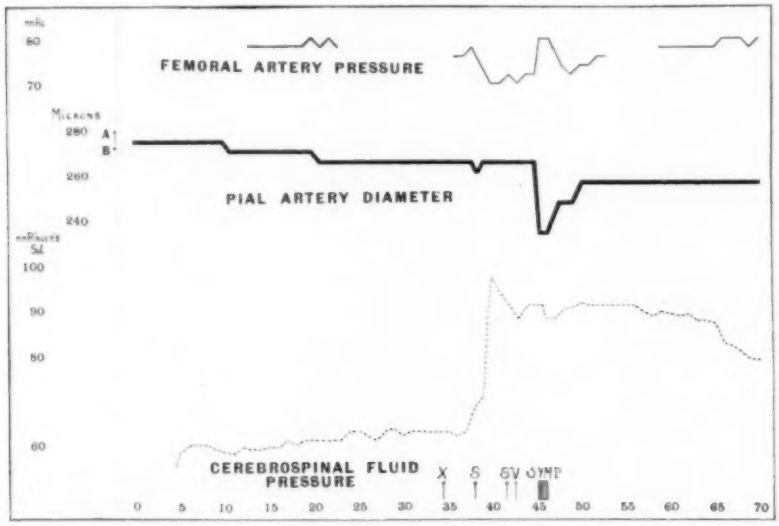


Fig. 4 (experiment 70).—Sympathetic stimulation. After a control period of forty-five minutes a one minute faradic stimulation (coil distance 12.5 cm.) of the left cervical sympathetic nerve was started. Constriction of the pial artery was visible within fifteen seconds, and in half a minute this amounted to 11.6 per cent of its previous size. The arterial pressure rose and the cerebrospinal fluid pressure fell slightly during the constriction. Then the artery slowly (within four minutes) returned to a diameter slightly less than its initial diameter, and remained unchanged for a second control period of twenty-five minutes. At the time indicated by the arrow (*X*) at the bottom of the chart, artificial respiration by intermittent air blast through the tracheal cannula was started. Shortly after this the cerebrospinal fluid pressure rose abruptly and maintained a height of approximately 30 mm. of Ringer's solution higher than its previous level. The second arrow (*S*) indicates manipulation of the cervical sympathetic nerve for a few moments previous to the electrical stimulation later. A slight constriction of the pial artery was observed during this mechanical stimulation of the nerve. The arrows (*S'* and *V*) indicate times when the left sympathetic and left vagus were cut.

these cases should be considered separately from those in which the constriction occurred in the face of a constant or a rising blood pressure.

Vagus stimulation was followed by dilatation of pial arteries, of the same size as those just mentioned, in twelve of the thirteen animals examined (figs. 6, 7 and 8 and table 2). A sudden and often striking fall in general systemic arterial pressure occurred in almost every case. Of the thirty-five trials, 97 per cent showed dilatation of 9 microns or slightly more, and 87 per cent, dilatation of much more than

TABLE 1.—*Stimulations of Sympathetic Nerves* *

Number of Animals	23
Number of Animals showing Constriction	23
Total Number of Stimulations	89
I. With no change or a rise in blood pressure †	
Stimulations	75
Constrictions	69
Dilatations	0
No change	6
The extent of change in diameter which could be accurately measured was 9 microns.	
In group I constrictions of more than 9 microns	60%
constrictions of more than 18 microns	29%
constrictions of more than 27 microns	13%
II. With a fall in blood pressure of more than 10 mm. of mercury.	
Stimulations	14
Constrictions	10
Dilatations	2
No change	2

* Changes in diameter of pial arteries measured directly with micrometer ocular. The size of the arteries ranges from 108 to 342 microns. Magnification, 80.

† A moderate rise in blood pressure was observed in most cases in this group which includes also thirteen instances of a slight fall in blood pressure (less than 10 mm. of mercury). In most of these the difference in pressure was so near the observational error that a separate classification did not seem warranted.

that amount. Photomicrographs (fig. 9) show characteristic dilatation after vagus stimulation. The degree of dilatation varied from 3 to 50 per cent of the artery's initial diameter. In some cases a slight transient constriction preceded the dilatation (fig. 5). This was probably caused by the sudden big fall in blood pressure which occurred in these cases. The latent period was approximately the same as that following sympathetic stimulation. Photomicrographs (fig. 10) show constriction after sympathetic stimulation, followed by recovery, and then a slight addi-

tional dilatation after vagus stimulation. With variations in the strength of the current (as measured by inductorium coil distance), striking differences in responses were not noted. After prolonged exposure of the nerves, however, the response became less, and then a stronger current caused a somewhat greater response. Differences in the size of the arteries seemed to affect somewhat the degree of response to stimulation, the smaller vessels showing the greater response (table 5).

It is interesting to note that the data obtained by an entirely different method—that of measuring the diameters of pial arteries from the

TABLE 2.—*Stimulations of Vagus Nerve* *

Number of Animals	13
Number of Animals showing Dilatation	12
Total Number of Stimulations	35
I. Without any change in blood pressure †	
Stimulations	5
Constrictions	0
Dilatations	3
No change	2
II. With a fall in blood pressure of more than 10 mm. of mercury.	
Stimulations	30
Constrictions	0
Dilatations	28
No change	2
The extent of change in diameter which could be accurately measured, 9 microns.	
In group II, dilatation of more than 9 microns	87%
dilatations of more than 18 microns	63%
dilatations of more than 27 microns	37%

* Changes in diameter of pial arteries measured directly with micrometer ocular. The size of the arteries ranges from 108 to 333 microns. Magnification, 80.

† This group includes cases with a slight fall in blood pressure, the difference in pressure being close to the observational error, and in no case more than 10 mm. of mercury.

negatives of photographic plates, exposed before and after the nerve stimulations—were in every way comparable with the data obtained by observation and micrometry (tables 1, 2 and 4). It is difficult to state the relative accuracy of the two methods, but on account of slight differences in focus, intervals between exposures, etc., the photographic method seemed definitely less accurate than that of direct micrometry. By both methods the same degree of constriction was found after sympathetic stimulation, and the same degree of dilatation after vagus stimulation.

So, also, in the experiments with epinephrine the two methods yielded comparable results (tables 3 and 4). Local irrigation with epinephrine (in the dilute solutions mentioned) beneath the cranial window, i. e., directly over the surface of the pia, was not attended by an appreciable change in systemic arterial pressure, but was followed by constriction of the pial artery (bathed by the epinephrine solution) in each of the eleven trials (table 3, and figs. 11, 12 and 13). Once, at least, constriction of a pial vein also resulted (fig. 13). Intravenous injection of epinephrine, on the other hand, often caused a great rise in systemic arterial pressure and, coincidentally, a dilatation of the pial arteries. When the blood pressure began to fall, though it was still far above the initial level, a true constriction of the pial artery (the diameter now being less than the initial) was noted in five of five trials (fig. 14). Injections of epinephrine into the carotid artery always caused a constriction of the pial artery, in spite of the coincident rise in blood

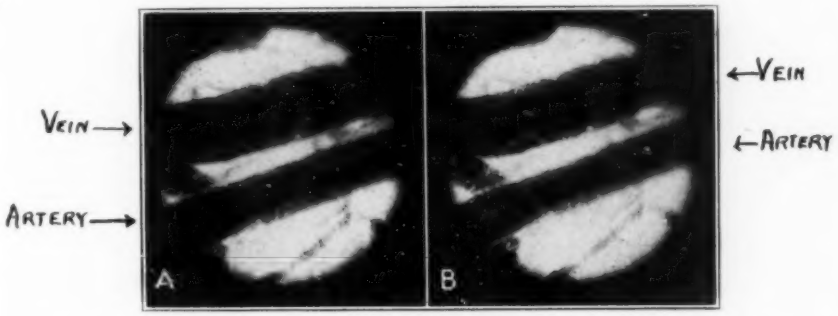


Fig. 5 (experiment 39).—Sympathetic stimulation. Actual magnification, 20 diameters. The exposures of five seconds' duration each show an artery and a vein enveloped by pia-arachnoid. *A*, four and a half minutes before stimulation. The artery is 117 microns in diameter. *B*, one-half minute after a sixty second galvanic stimulation of the left cervical sympathetic nerve. The artery shows a slight but definite constriction which amounts to 10.7 per cent of its initial diameter. The vein does not show measurable change. (The stimulating current, taken from one dry cell, was interrupted by a motor so as to give approximately forty shocks a second of one-fiftieth second each. This figure and figures 9, 10, 12 and 13 show blood vessels of the pia-arachnoid over the left parietal cortex of cats. The photographs were taken through a cranial window, the initial magnification being 20 diameters, except in one case, in which it was 8 diameters. The animals were anesthetized with a warm 1 per cent solution of iso-amyl-ethyl barbituric acid (usually 85 mg. per kilogram of body weight), injected intraperitoneally.

pressure (figs. 15 and 16). After injection into the carotid, the rise in arterial pressure was not so high as after intravenous injection, and of course the concentration of epinephrine within the cerebral arteries was far greater in the former.

Change in diameter of the pial arteries was not noted after stimulation of the vagus or the cervical sympathetic nerve on the opposite (right) side of the head from that of the observed vessel. It is interesting, however, that stimulation of the nerve either on the right or on the left was followed by an equal fall in intracranial pressure. These results are to be expected if response to stimulation of the nerve is unilateral, though our evidence (two trials in one animal) is too slight

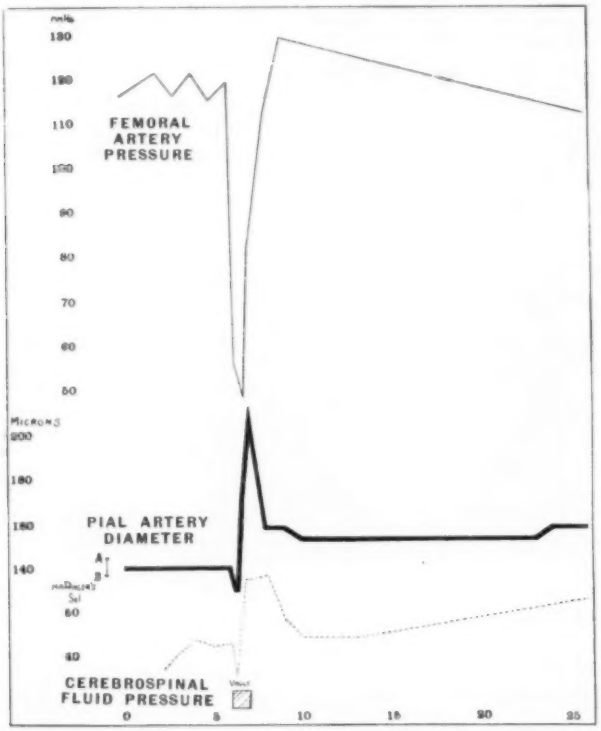


Fig. 6 (experiment 22).—Vagus stimulation. After a control period of six minutes a one minute faradic stimulation (coil distance 11 cm.) of the left vagus nerve was started. The arterial pressure abruptly fell 63 mm. of mercury. The pial artery constricted slightly for a few seconds and then, before the arterial pressure reached its lowest point, began to dilate and continued rapidly until its diameter was 50 per cent greater than its previous width, accompanied by a steep rise in arterial pressure. The pial artery then rapidly constricted to a point above its initial level while the arterial pressure rose quickly to a point just above its previous level. The cerebrospinal fluid pressure paralleled, roughly, the changes in diameter of the pial artery.

to justify any definite inference. Constriction of vessels on the right side of the brain should cause the same reduction in intracranial pressure as constriction of those on the left. Owing to the design of the head

TABLE 3.—*Experiments with Epinephrine*

Number of Animals, 14.	
Method:	
A.* Local (irrigation beneath window)	
Number of trials	12
Constrictions alone	9*†
Constrictions followed by dilatation	3
Dilatations alone	0
B.‡ Intravenous	
Number of injections	6
Constrictions alone	0
Constrictions after dilatation	4
Dilatations alone	2
C.§ Carotid	
Number of injections	3
Constrictions without dilatation	3
Dilatations alone	0
D. Intramuscular	
Number of injections	1
No satisfactory results (i.e., no evidence of action of epinephrine on blood pressure)	

* In this group a dilution of 1 : 10,000 was employed in seven cases, 1 : 250,000 in two cases, and 1 :500,000 in three cases.

† Constriction of a pial vein also noted once.

‡ In this group amounts varying from 0.1 to 0.6 cc. of a 1 :10,000 dilution were injected in five animals, and 1.8 cc. of 1 :100,000 in one.

§ In this group amounts varying from 0.4 to 0.5 cc. of a 1 :10 000 dilution were injected in two animals and 0.8 cc. of 1 :100,000 in one.

TABLE 4.—*Photomicrographic Changes in Diameter of Pial Arteries Measured from Negatives*

Sympathetic Stimulations—4 animals	
Stimulations	5
Constrictions	5
Vagus Stimulations—4 animals	
Stimulations	4
Constrictions	1
Dilatations	3
Use of Epinephrine	
Number of animals	3
Number of experiments	5
Local (irrigation beneath window)	
Constrictions	3
Dilatations	0
Intravenous	
Constrictions	1
Dilatations	1*

* Blood pressure rose 65 mm. of mercury in this case.

holder the cranial window has always been made on the left, so that the converse of this experiment—observation of pial vessels on the right side of the brain during stimulation of the cervical sympathetic nerve on the same and on the opposite side—has not been tried. Stimulation of the heart end of the vagus or the central end [20] of the sympathetic on the same side, or stimulation of the crural or of the sciatic nerves did not cause an appreciable change in arterial diameter.

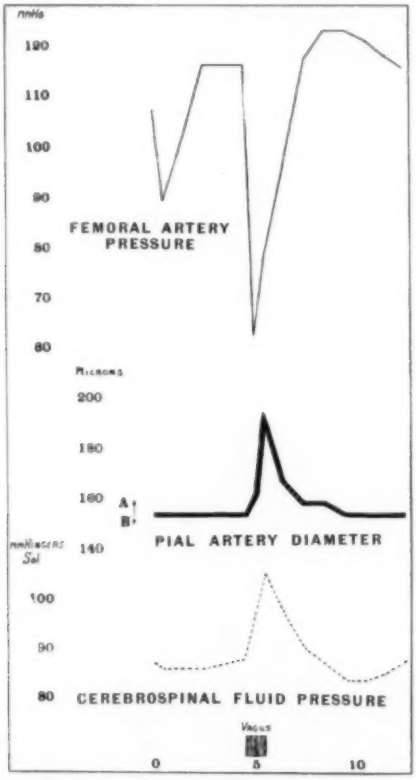


Fig. 7 (experiment 22).—Vagus stimulation. After a control period of four and one-half minutes a one minute faradic stimulation (coil distance 12 cm.) of the left vagus nerve was started. The arterial pressure fell 54 mm. of mercury and the pial artery dilated, first slowly and then rapidly, reaching a diameter 25.6 per cent greater than its previous width. Then, while the arterial pressure rose to a point just above its previous level, the pial artery constricted to its original size. The cerebrospinal fluid pressure paralleled the changes in diameter of the pial artery.

Twenty nerve stimulations in eleven different animals were excluded for the following reasons: (1) failure of the electrode to remain in

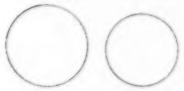
20. Central end is used to designate the fibers running between the point of section and the spinal cord.

place on the nerve; (2) insufficient data; (3) irregular base-line (i. e., the artery changing in diameter owing to causes other than the known experimental procedures); (4) changes in diameter occurring after two minutes from the beginning of the stimulation; (5) the animal in poor condition (shivering, extremely low blood pressure or cyanosis).

COMMENT

Difficulty has been experienced in controlling the many variables which make interpretations of intracranial measurement so hazardous, and it may be well to mention the precautions which have been taken in the experiments here described to avoid error from many sources. Measurements of the pial arteries throughout control periods (during

TABLE 5.—*Pial Arteries Grouped According to Size—Comparative Changes in the Different Groups After Nerve Stimulation (in Terms of Per Cent of Initial Size)*

Sympathetic		Vagus	
Group of Large Arteries (From 252 to 342 microns in diameter)	Group of Small Arteries (From 108 to 247 microns in diameter)	Group of Large Arteries (From 220 to 333 microns in diameter)	Group of Small Arteries (From 108 to 216 microns in diameter)
Average Areas: Before / After	Average Areas: Before / After	Average Areas: Before / After	Average Areas: Before / After
			
Average reduction in area .....14.4% Average reduction in diameter 7.0%	Average reduction in area .....16.0% Average reduction in diameter 8.5%	Average increase in area* .....19.8% Average increase in diameter 9.0%	Average increase in area† .....49% Average increase in diameter 22%
		* Equal to a change of 15 per cent of the larger final area.	† Equal to a change of 33 per cent of the larger final area.

which additional experimental procedures were not instituted) were recorded nine times for more than half an hour, and twice, for more than an hour. The measurements were usually made every minute. In four of these control periods the vagi and sympathetic nerves on both sides of the neck had been tied or cut. Usually, only slight variations in arterial diameter occurred during these periods, and ordinarily a base line of a few minutes sufficed to determine whether accidental variations were going on. Occasionally, toward the end of a long experiment, when the nerves in the neck had been cut, large spontaneous changes in arterial diameter were observed. Results of stimulation during and after such periods were discarded.

Normal changes in intravascular pressure that occurred with different phases of the cardiac or respiratory cycles might be held responsible for the recorded changes in arterial diameters which we

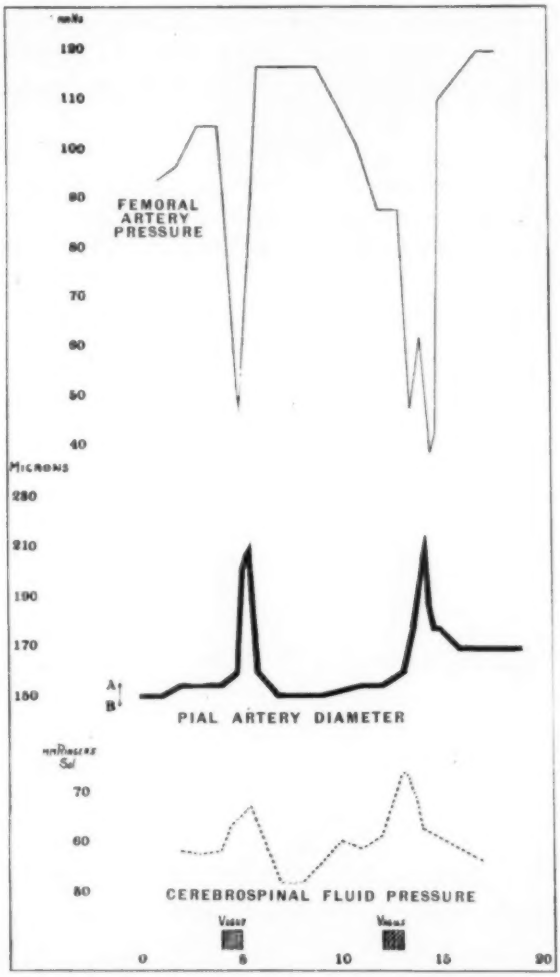


Fig. 8 (experiment 22).—Vagus stimulation. After a control period of four minutes a one minute faradic stimulation (coil distance 11 cm.) of the left vagus nerve was started. The arterial pressure fell 58 mm. of mercury. The pial artery dilated until its diameter was 36 per cent greater than its previous width. Then, while the pial artery constricted to its original size, the arterial pressure rose abruptly to a point above its initial level. After an interval of five minutes another one minute faradic stimulation (coil distance 11 cm.) was started. A fall in arterial pressure and dilatation of the pial artery was again observed, similar in extent to the earlier ones. The cerebrospinal fluid pressure roughly paralleled the changes in diameter of the pial artery. (Artificial respiration was started and continued during and after the periods of stimulation.)

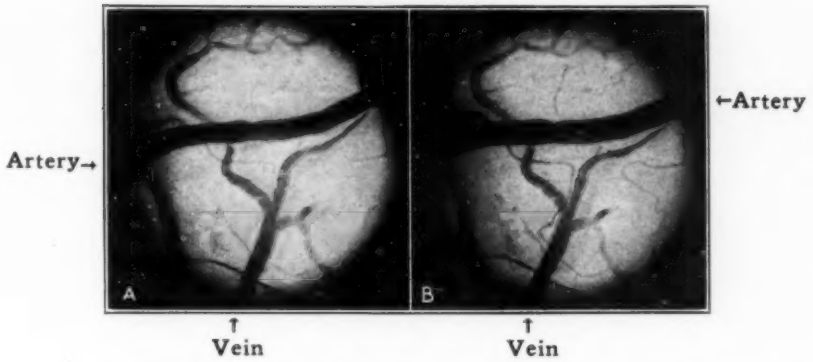


Fig. 9 (experiment 22).—Vagus stimulation. Actual magnification, 16 diameters. *A*, exposure of ten seconds, nine minutes, before vagus stimulation. The artery is 250 microns in diameter. *B*, exposure of twelve seconds, at the end of a sixty second faradic stimulation of the left vagus nerve (coil distance 12 cm.). The artery shows evident dilatation, which amounts to 25 per cent of its initial diameter. The vein does not show measurable change. (The local indentation of the upper edge of the artery is an artefact, not an annular constriction.)

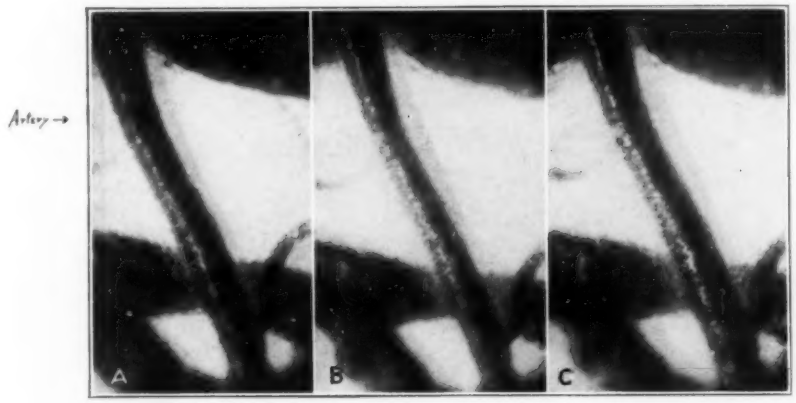


Fig. 10 (experiment 52).—Sympathetic and vagus stimulation. Actual magnification, 38 diameters. Exposures each of five seconds' duration. *A*, two minutes before sympathetic stimulation. The artery is 58 microns in diameter. *B*, forty seconds after a sixty second faradic stimulation of the left cervical sympathetic nerve (coil distance 9 cm.). The artery shows a constriction amounting to 15 per cent of its initial diameter. *C*, at the end of a sixty second faradic stimulation of the left vagus nerve (coil distance 9 cm.). The artery has dilated to 63 microns in diameter, which is 7 per cent larger than before the sympathetic stimulation.

have attributed to special experimental conditions: nerve excitation and the action of epinephrine. The photographic plates, however, were sometimes exposed for fifteen seconds, ample time to include many heart beats and several respirations. On the most careful examination with a magnification of 200 diameters, sudden rhythmic changes in diameter of the pial arteries paralleling heart beat or respiration were not visible. Moreover, the slow changes in diameter of the arteries after nerve stimulation always lasted for more than thirty seconds.

Two other possible sources of error were considered—cerebral "anemia" due to a sudden extreme fall in arterial pressure, and changes

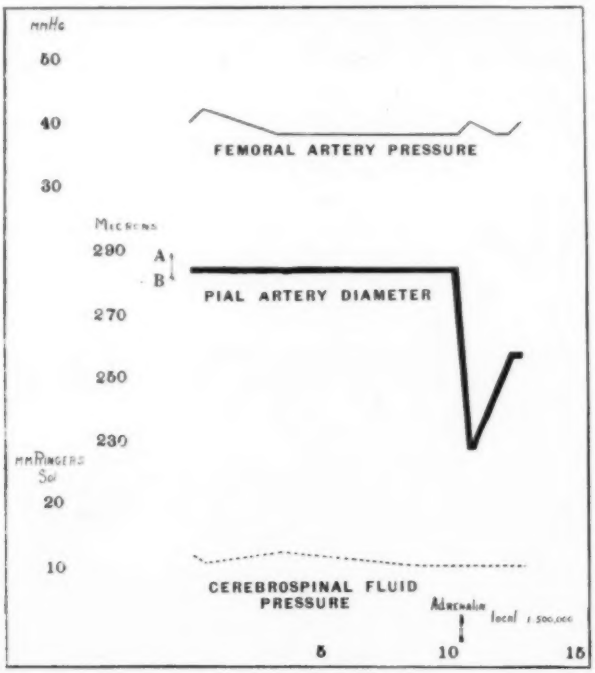


Fig. 11 (experiment 18*a*).—Epinephrine injected locally while the animal was under ether anesthesia. After a control period of ten and a half minutes, a 1 : 500,000 solution of epinephrine in Ringer's solution was injected beneath the cranial window, replacing an equivalent amount of cerebrospinal fluid. The pial artery constricted to a diameter 18.7 per cent of its initial width and then slowly returned toward its previous size. The arterial and cerebrospinal fluid pressures did not show an appreciable change.

in oxygen or carbon dioxide content of the blood due to considerable changes in rate or in depth of respiration. Sudden "anemia" (in the foregoing sense), low oxygen and high carbon dioxide content all cause dilatation of the pial vessels.[14] In many instances of vagus stimulation an abrupt fall in arterial pressure occurred (figs. 6, 7 and 8)

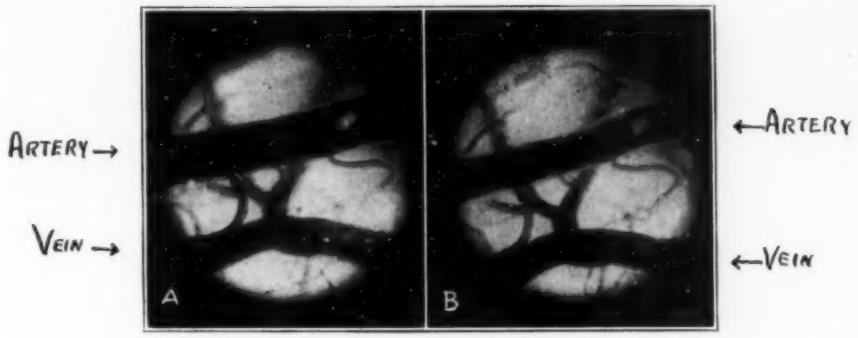


Fig. 12 (experiment 34).—Injection of epinephrine. Actual magnification, 20 diameters. Exposures each of two seconds' duration. *A*, three minutes before. The artery is 112 microns in diameter. *B*, six minutes after a 1 : 100,000 solution of epinephrine in warm Ringer's solution was injected beneath the cranial window replacing an equal amount of cerebrospinal fluid. The artery shows a constriction which amounts to 24 per cent of its initial diameter. The vein does not show measurable change. (The oval light spot on the artery is an artefact.)

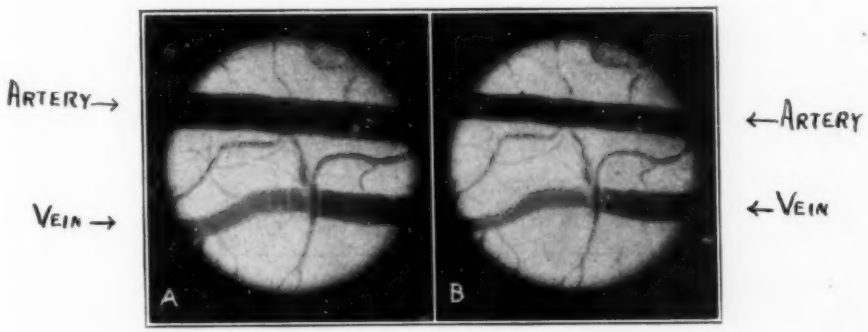


Fig. 13 (experiment 38).—Injection of epinephrine. Actual magnification, 20 diameters. *A*, four minutes before injection of epinephrine. The artery is 85 microns in diameter. *B*, one minute after a 1 : 10,000 solution of epinephrine in warm Ringer's solution was injected beneath the cranial window. The artery shows a constriction amounting to 24 per cent of its initial diameter. The vein shows at one point a constriction of about 23 per cent of its initial diameter.

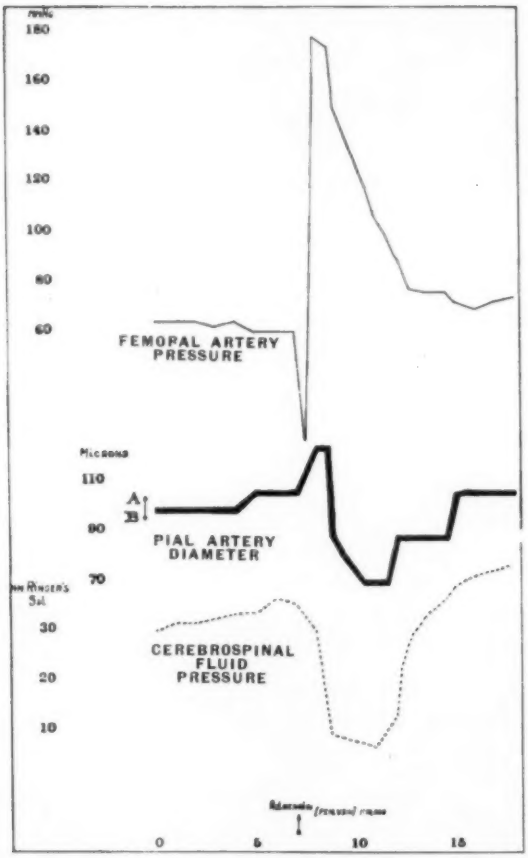


Fig. 14 (experiment 2).—Epinephrine injected into vein. After a seven minute control period, a 1 : 10,000 solution of epinephrine in Ringer's solution, 1 cc., was injected into the saphenous vein. The arterial pressure at once fell 44 mm. of mercury, and then, within a minute, rose to a point 118 mm. of mercury above its previous level. The pial artery dilated during this period to a diameter 33.3 per cent of its original width, and then constricted quickly at the same time that the arterial pressure began to fall. The pial artery had reached the extreme point of its constriction when the arterial pressure was still 60 mm. of mercury above its original level. After a minute the pial vessel began to dilate while the arterial pressure was still falling. The cerebrospinal fluid pressure roughly paralleled the changes in diameter of the pial artery.

and the dilatation might have been explained as secondary to the sudden "anemia" thus caused; however, in five other instances, the arterial pressure fell slightly (12 mm. of mercury or less), and the dilatation of the artery still occurred. In regard to the question of variability of blood oxygen or carbon dioxide content, it is true that alterations in respiratory rate and depth often follow vagus or sympathetic stimulations. To avoid error from this source artificial respiration was

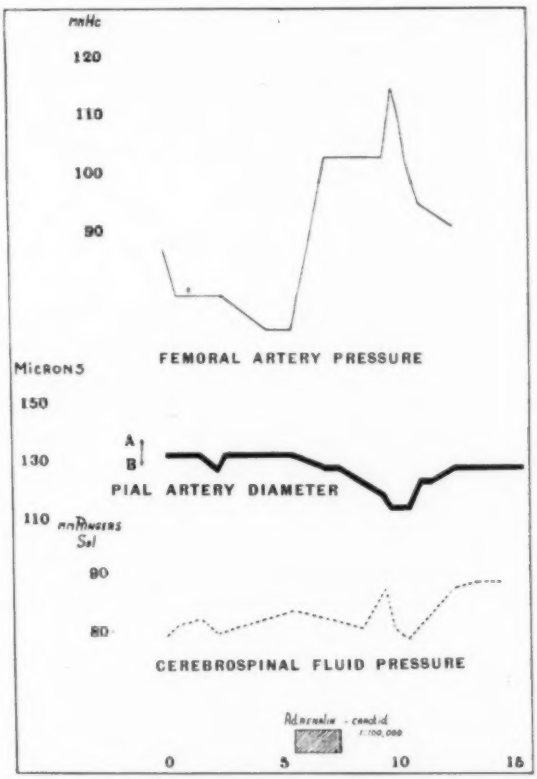


Fig. 15 (experiment 8*a*).—Epinephrine injected into left carotid. After a five and a half minute control period, a 1:100,000 solution of epinephrine in Ringer's solution, 0.8 cc., was injected over a period of two minutes into the left carotid. The arterial pressure rose 42 mm. of mercury and then fell. The pial artery constricted slowly to a diameter 15.5 per cent less than its initial width, while the arterial pressure rose, and returned nearly to its normal size while the arterial pressure was falling. The cerebrospinal fluid pressure followed the changes in arterial diameter except for a slight rise while the artery was still constricting.

started before the stimulation, and was continued throughout the period of recorded change in vessel caliber in eleven instances. The result was the same as when artificial respiration was not used.

Leonard Hill and others have maintained that changes in venous pressure, transmitted through the capillaries, might cause changes in

size of the cerebral arteries. In order to determine whether in our experiments changes in venous pressure actually were responsible for the observed changes in arterial caliber, after sympathetic and vagus stimulation, pressure in the sagittal sinus was measured in one animal coincidentally with the usual measurements of pial artery diameter,

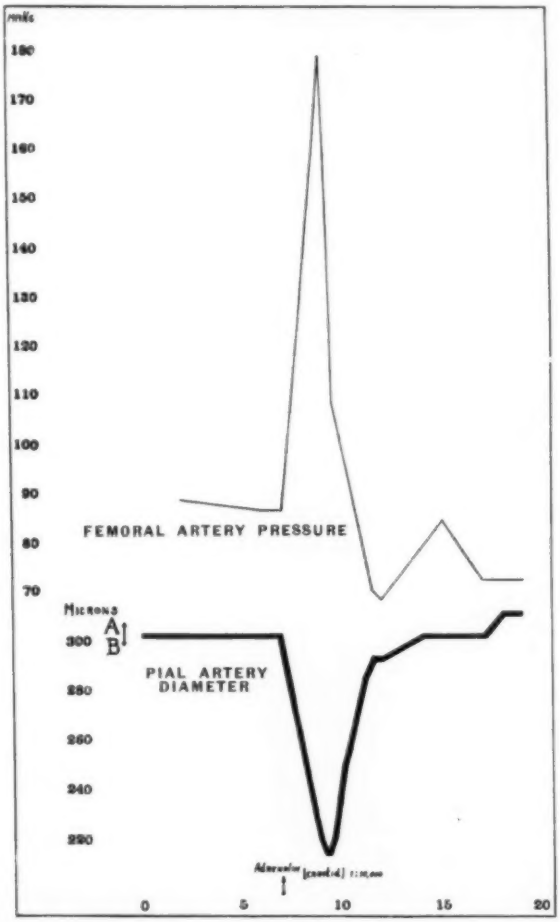


Fig. 16 (experiment 8*b*).—Epinephrine injected into left carotid. After a control period of seven minutes, a 1 : 10,000 solution of epinephrine in Ringer's solution, 0.5 cc., was injected into the left carotid. The arterial pressure rose 92 mm. of mercury and simultaneously the pial artery constricted to a diameter 29.4 per cent of its initial width, and then quickly regained its original size while the arterial pressure fell abruptly to a point below its original level.

cerebrospinal fluid pressure and femoral arterial pressure. The change in intracranial venous pressure, though increasing with increase in caliber of the pial arteries, was only 4 mm. of water in extent. At another

period in the same experiment, spontaneous changes in intracranial venous pressure of the same magnitude occurred without change in size of the pial artery.

Variations in cerebrospinal fluid pressure might be thought to cause changes in size of the pial arteries, but evidence in favor of this has not been found. On the contrary, we have found [21] by trial that raising the cerebrospinal fluid pressure (by addition of fluid through the cisterna magna) did not produce an effect on diameters of the pial arteries until a height was reached which was many times that observed during the nerve stimulation experiments.

Sudden changes in physical or chemical composition of the blood, as by addition of hypertonic or hypotonic solutions, acids, bases, etc., cause changes in diameter of the pial arteries. (The effect of such solutions will be discussed in a later paper.) A rise in systemic arterial pressure often accompanies constriction of pial arteries and a fall in pressure accompanies dilatation of these arteries. The first condition was found in 39 per cent of our experiments in which the sympathetic nerves were stimulated; the latter, in 86 per cent of those in which the vagus nerve was stimulated. It is possible that anemia or hyperemia of the vasomotor regulatory apparatus may cause these variations in systemic blood pressure. Such an explanation is consistent with the observations of Cushing,[21a] who showed that sudden interference with the circulation of the vasomotor center results in a rise in systemic arterial pressure, and with the experiments in crossed circulation of Anrep and Starling,[22] who showed that a fall in cerebral arterial pressure caused a rise in systemic arterial pressure, and vice versa. More recently, Gesell's [23] results, reached by a different method of approach, are also in accord. He found that a more rapid flow of blood through the head was accompanied by a less rapid flow through the extremities and vice versa.

Dilatation of pial arteries that has resulted from stimulation of the vagus nerve is evidently due to actual dilator impulses, not to inhibition of constrictor impulses, for the cervical sympathetic nerves on both sides of the neck were cut previous to the stimulation.

Passive changes in caliber of the cerebral arteries, i. e., mechanical expansion or collapse following sudden and extreme changes in systemic arterial pressure, have often been observed, and these passive changes

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21. Wolff, H. G., and Forbes, H. S.: Arch. Neurol. & Psychiat., to be published.

21a. Cushing, H.: Am. J. M. Sc. **124**:376, 1902.

22. Anrep, G. V., and Starling, E. H.: Proc. Roy. Soc. London, series b **97**: 463, 1924-1925.

23. Bronk, D. W., and Gesell, R.: Proc. Soc. Exper. Biol. & Med. **24**:257, 1926.

may obscure or alter vasomotor changes occurring at the same time (figs. 6 and 14). Due allowance, as previously mentioned, has been made for changes of this nature.

SUMMARY AND CONCLUSION

By direct micrometry and photomicrography, together with simultaneous measurements of cerebrospinal fluid pressure and of intracranial and extracranial vascular pressures, evidence has been secured that the circulation of the brain is not regulated wholly from a distance by splanchnic or systemic vasomotor control. On the contrary, many influences share in this vital regulation. Thus it has been found that, although the changes in caliber of the cerebral arteries may passively follow sharp fluctuations in systemic arterial pressure, constriction of arteries also follows direct application of epinephrine or stimulation of the cervical sympathetic nerves whereas dilatation follows stimulation of the vagus.

In conclusion, it seems justifiable to state that evidence of a quantitative nature has been obtained that the circulation of the mammalian brain is controlled in part by cerebral vasomotor nerves.

APPENDIX

RÉSUMÉ OF THE LITERATURE ON CEREBRAL VASOMOTOR PHENOMENA

1. *A. Direct Observation.*—Brachet[24] (1830) noted congestion of the cerebral cortex after evulsion of the superior sympathetic ganglia. Schiff[25] (1855) could not substantiate Brachet's observations. Through an open trephine hole and without anesthetizing the animals, Van der Beke Callenfels[26] (1855) found in two of twelve rabbits that stimulation of the cervical sympathetic nerve caused homolateral constriction of the pial arteries, followed by dilatation. Ackermann[27] (1858), using a skull window, found (experiment 32) that section of the cervical sympathetic nerve caused dilatation of the vessels on the same side and that stimulation caused constriction. Donders[28] (1859) cemented a glass window into the skull of rabbits and observed the pial vessels with a microscope that had a magnification of 45 diameters. On stimulation of the sympathetic nerve of the neck, he observed constriction of pial arteries followed by dilatation. Schultz[29] (1866) could not see any changes in the pial vessels after stimulation of the cervical sympathetic nerve. Without employing anesthesia or the window in rabbits, Nothnagel[30] (1867) noted (experiment 1) dilatation of the pial arteries

24. Brachet, J. L.: Recherches expérimentales sur les fonctions du système nerveux ganglionnaire et son application à la pathologie, Paris, Germer-Baillière, 1837.
25. Footnote 5, first reference.
26. Footnote 6, first reference.
27. Ackerman, T.: Virchows Arch. f. path. Anat. **15**:401, 1858.
28. Footnote 9, first reference.
29. Schultz, A.: Petersb. med. Ztschr., 1866, p. 11.
30. Footnote 6, second reference.

on the side of the transsected sympathetic nerve of the neck. Faradic stimulation of these nerves (experiment 2) caused constriction of the pial arteries. Stimulation of sensory nerves similarly caused constriction. Von Goujon[31] (1867) thought that he found pial congestion and inflammation in animals killed after cervical sympathetic section, but his experiments were poorly controlled. Riegal and Jolly[32] (1871), using a window,[33] with and without anesthesia, were unable to confirm Nothnagel's observations. [The following experimentalists also contributed to the development of the skull window as a means of investigation of brain circulation: Ravina[34] (1811), Berlin[35] (1851), Kussmaul and Tenner[36] (1875), Leyden[37] (1866), Jolly[32] (1871), Elder[38] (1897), Cushing[21] (1902), Lewin[39] (1920) and Lee[40] (1925).] After many unsuccessful trials with rabbits Krauspe[41] (1874) was finally able in a few instances to produce constriction of the pial arteries on faradic stimulation of the crural nerve. Schuller[42] (1874), using a magnifying glass but not employing the window, noted homolateral cerebral arterial constriction on stimulation of the cervical sympathetic nerves. He observed similar constriction when he irritated the skin with a mustard inunction. By direct inspection of the cerebral arteries through an open trephine hole, Vulpian[43] (1875) saw, on stimulation of the cervical sympathetic nerves, constriction preceded by dilatation. Meyer and Pribram[44] (1875) saw constriction of the pial arteries when the stomach was stimulated. Spina[45] (1893) observed dilatation of cerebral arteries after tetanization of the intact medulla and after section through the hindbrain. Von Cyon[46] (1899) watched the pial arteries through a hole in the skull. After intravenous injection of suprarenal extract, at first there was a temporary vasodilatation, during the rise of the systemic arterial pressure, followed by a vasoconstriction at the height of the rise in blood pressure. Hill and Macleod[3] (1900) observed the cerebral vessels through a trephine hole in the skull. The response of the cerebral vessels to epinephrine was compared with the response of the vessels of the mesentery. Even with a magnifying glass, a change could not be seen in the size of the pial arteries. After the same stimulus the mesenteric vessels showed obvious constriction. Cushing[21] (1902) saw blanching of the surface of the brain after the local application of epinephrine. Hirschfelder[47] (1914), comparing the reac-

31. Von Goujon, E.: J. de l'anat. et phys. **4**:106, 1867.

32. Jolly, F.: Untersuchungen über den Hirndruck und die Blutbewegung im-Schädel, Wurzburg, 1871.

33. Footnotes 21, 32, 34, 35, 36, 37, 38, 39 and 40.

34. Ravina, A. F.: Mem. de l'acad. des sc. de Turin, 1811, p. 61.

35. Berlin, W., in Schmidt's Jahrbuch **69**:14, 1851.

36. Kussmaul, A., and Tenner, A.: Untersuchungen zur Natur Lehre des Menschen und der Thiere, Frankfurt, Meidinger Son & Co., 1857.

37. Leyden, E.: Virchows Arch. f. path. Anat. **37**:519, 1866.

38. Elder, G.: Brit. M. J. **2**:1414, 1897.

39. Lewin, L.: Kohlenoxydvergiftung, Berlin, Julius Springer & Co., 1920.

40. Lee, F. C.: Am. J. Physiol. **74**:317, 1925.

41. Krauspe, M. F.: Virchows Arch. f. path. Anat. **59**:472, 1874.

42. Schuller, M.: Berl. klin. Wchnschr., 1874, p. 294.

43. Footnote 7, second reference.

44. Meyer, A., and Pribram, E., quoted from Rosenthal: Klinik der Nervenkrankheiten, Stuttgart, Ferdinand Enke, 1875, p. 97.

45. Spina, A.: Wien. Medblätter. **21**:247, 1898; Arch. f. d. ges. Physiol. **76**: 204, 1899.

46. Von Cyon, E.: Arch. f. d. ges. Physiol. **74**:97, 1899.

47. Footnote 8, second reference.

tions of the retinal and pial arteries to drugs and observing the minute vessels as well, measured the systemic blood pressure in the carotid artery. Active vasoconstriction occurred in both pial and retinal arteries after injection of epinephrine. Florey [4] (1925), who examined the pial vessels through an open trephine hole, could not find any change in size after stimulation of the cervical sympathetic nerves, vasomotor centers, or stellate ganglia. Jacobi and Magnus [48] (1925), with a microscope of 13.5 diameters' magnification, photographed pial vessels of dogs through an open trephine hole. They noted vasoconstriction after pain and after intracarotid injections of epinephrine. In microscopic studies of cerebral arteries at the base and over the convexity of frog brains, Sandor [49] (1926) noted vasoconstriction after intracarotid injection of epinephrine and dilatation after intravenous injection.

B. Retinal Studies.—Gerhardt [50] (1900) studied the cerebral circulation through variations in pressure of the cerebral venous outflow. He also observed the retinal vessels after intravenous injections of epinephrine. He saw only dilatation associated with the rise in systemic blood pressure, and concluded that constriction of the pial or retinal arteries does not occur after administration of this drug. Kahn [51] (1904) also studied the retinal vessels and noted that epinephrine injected into the arteries of the head caused constriction of the retinal vessels, but that when it was given intravenously only dilatation occurred. He observed constant vasoconstriction of the retinal arteries on faradic stimulation of the cervical sympathetic nerves in rabbits but failed ever to observe such changes in the retinal arteries of cats or of monkeys. Hirschfelder [52] (1914), as already mentioned, observed a parallelism in the retinal and pial arteries in their response to epinephrine.

2. *A. Studies of Pressure and Speed of Flow Bearing Directly on Vasomotor Problems.*—Cramer [53] (1873) measured the venous pressure in the internal jugular veins and used it as an index of cerebral blood flow. On painful stimulation of a peripheral nerve and faradic stimulation of the cervical sympathetic nerves, he obtained inconstant results and hesitated to draw conclusions concerning cerebral vasoconstriction. Gaertner and Wagner [54] (1887) measured the rate of venous outflow in dogs. They studied the effect of stimulating the vasoconstrictor center by strychnine and by asphyxia and concluded that the cerebral arteries dilated passively as the result of great elevation in systemic blood pressure. Reiner and Schnitzler [55] (1897), using a similar method, noted a great increase in outflow after faradic stimulation of the vagosympathetic in a dog. Von Cyon [46] (1899) noted that stimulation of the central end of the vagus caused an increase in cerebral venous outflow. Pick [56] (1899), measuring arterial blood pressure and venous outflow, believed that epinephrine could cause cerebral vasoconstriction.

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48. Jacobi, W., and Magnus, G.: Arch. d. Psychiat. u. Nervenkrankh. **74**:126, 1923.

49. Sandor, G.: Arch. f. d. ges. Physiol. **213**:492, 1926.

50. Footnote 5, fourth reference.

51. Kahn, R. H.: Centralbl. f. Physiol. **18**:153, 1904.

52. Footnote 8, second reference.

53. Cramer, P.: Inaugural Address, Dorpat, 1873.

54. Footnote 5, third reference.

55. Reiner, M., and Schnitzler, T.: Arch. f. exper. Path. u. Pharmakol. **38**:249, 1896-1897.

56. Pick, F.: Arch. f. exper. path. u. Pharmakol. **42**:399, 1899.

Neujean [57] (1904) measured the systemic arterial blood pressure in the heart end of the ligated internal carotid and also the cerebral venous outflow. He concluded that epinephrine injected into the carotid artery could cause cerebral vasoconstriction (see also Berezin [58] 1916). Yamakita [59] (1922), measuring the venous outflow through the superficial temporary artery of rabbits, observed an increase in flow immediately after the injection of epinephrine during the rise in blood pressure, and later a decrease in flow. Miwa, Ozaki and Shiroshita [60] (1927) used a compensator in rabbits to prevent changes of a passive nature in the blood vessels of the brain during the rise in systemic pressure, and noted a decrease in venous output after injection of epinephrine.

B. Measurement of Variations in Brain Volume.—Von Schulten [61] (1884), using rabbits, developed a combined method. He measured cerebrospinal fluid pressure through the cistern and brain volume by means of a hook attached to the meninges over the convexity. Variations in volume could be observed from the rise and fall of recorders attached to the hook. At the same time he made a few measurements, by stromuhr, of the speed of flow in the internal carotid artery and concluded that faradic stimulation of the cervical sympathetic nerves caused constriction of the cerebral arteries. Evidence of alteration in size of the cerebral arteries was noted when these nerves were merely transsected. Dean [62] (1892), measuring systemic arterial blood pressure and cerebrospinal fluid pressure in the spinal canal (without measurements of venous pressure), stimulated the vagosympathetic nerves and did not note any change in pressures consistent with cerebral vasoconstriction or dilatation. Werthheimer [63] (1893), using similar methods, came to similar conclusions. Roy and Sherrington [1] (1890) used the skull as an oncometer and adjusted a piston in a trephine hole over the cerebral convexity so that it rested on the surface of the brain. They also measured venous and arterial systemic pressures. An increase in brain volume after stimulation of the central end of the vagus was observed, but it was believed that this was due to the associated rise in venous pressure and not to active changes in the diameter of the cerebral arteries. They concluded that cerebral arteries did not possess active vasomotor properties. Muller and Siebeck [64] (1907), using a modification of the Roy and Sherrington method and in addition measuring the outflow of the cerebral venous blood, concluded that faradic stimulation of the central end of the vagus caused an increase in the brain volume as noted by Roy and Sherrington. In addition, they decided that, since there was an increase in outflow of cerebral venous blood, an increase in diameter of the cerebral arteries did actually occur. From this and other experiments they concluded that vasomotor phenomena do exist within the skull. E. Weber [65] (1908) also used a modification of the method of Roy and Sherrington. He modified the oncometer slightly to permit freer discharge of excess cerebrospinal fluid accumulating between the surface of the brain and the piston, and

57. Neujean, V.: Arch. internat. de Pharmakol. **13**:45, 1904-1905.

58. Berezin, I.: Russk. Vrach, vol. 15, no. 22, p. 513; abstr. in J. A. M. A. **67**:844 (Sept. 9) 1916.

59. Footnote 9, second reference.

60. Footnote 9, third reference.

61. Von Schulten, M. W.: Arch. f. Ophth. **30**:59, 1884.

62. Dean, H. P.: J. Path. & Bacteriol. **1**:26, 1892.

63. Wertheimer, E.: Arch. de phys. norm. et path. **5**:297, 1893.

64. Footnote 6, fourth reference.

65. Footnote 6, fifth reference.

he also measured the systemic arterial pressure. His observations and conclusions agreed with those of Muller and Siebeck. Weber emphasized the variability of the content of the cervical sympathetic nerves; occasionally dilatation occurred, and in about 25 per cent of the cases not any changes at all occurred.

C. Measurements of Variations in Pressure in the Circle of Willis.—Hurthle[66] (1889) put a cannula into the heart end and another into the cerebral end of a ligated internal carotid. He noted that stimulation of the cervical sympathetic nerves caused an increase of pressure in the cerebral end, which he interpreted as evidence of cerebral vasoconstriction. Arloing[67] (1889), Francois Franck[68] (1899), and Cavazzani[69] (1893), using similar methods, came to the same conclusion. Biedl and Reiner[70] (1900) measured the pressure in the circle of Willis and also the cerebral venous outflow. They concluded that epinephrine injected into the carotid artery caused cerebral vasoconstriction. This was not apparent, however, with intravenous injection. In these experiments, faradic stimulation of the vagosympathetic nerves in many cases caused changes which were interpreted to be the result of cerebral vasoconstriction. Wiechowsky[71] (1902), using a method similar to that of Hurthle, concluded from his experiments that faradic stimulation of the head end of the cervical sympathetic nerves caused cerebral vasoconstriction. In those few cases in which he did not get a response he always obtained evidence of constriction by applying the electrode directly to the superior sympathetic ganglion.

D. Combined Measurements of Arterial and Venous Pressure.—Bayliss and Hill[2] (1895) measured variation in pressures in the carotid, torcular herophili and right auricle and concluded that stimulation of the vagosympathetic nerves did not produce an alteration in the size of the cerebral arteries and that the changes that did occur were passive and resulted from rises or falls in the systemic pressures. Hill[72] (1896) measured the pressures in the carotid, the general venous pressure from the right auricle, the cerebral venous pressure in the torcular and the cerebrospinal fluid pressure through a trephine hole in the atlas. He did not alter his previously stated conclusions concerning the lack of effect on cerebral arteries of stimulation of the vagosympathetic nerves. Hill and Macleod[5] (1900), through measurements of the volume of the brain with coincident measurements of pressure in the heart end and in the head end of a ligated carotid, again substantiated the earlier conclusion of Bayliss and Hill.[2]

E. Speed of Arterial Flow.—Von Schulten[61] (1884) has been referred to in paragraph 2 *B*. Jensen[73] (1904) measured the speed of flow in the internal carotid artery by means of a stromuhr. He observed a decrease when the homolateral cervical sympathetic was stimulated. He concluded that this resulted from vasoconstriction of the cerebral arteries. By means of cross circulation prepara-

66. Footnote 6, third reference.

67. Arloing, M. S.: Arch. de physiol. norm. et path. **21**:115, 1889.

68. Franck, F.: Arch. de physiol. **25**:729, 1893; quoted from Tigerstedt (1923), who also mentions Knoll (Sitzungsb. d. k. Akad. d. Wissenach., 1886, vol. 93), among those who could not find evidence of vasoconstrictor nerves on cerebral vessels.

69. Cavazzani, E: Arch. ital. de biol. **19**:214, 1893.

70. Biedl, H., and Reiner, M.: Arch. f. d. ges. Physiol. **79**:158, 1900.

71. Wiechowsky, W.: Arch. f. exper. Path. u. Pharmakol. **48**:376, 1902.

72. Hill, L.: Physiology and Pathology of Cerebral Circulation, London, J. and A. Churchill, 1896.

73. Jensen, P.: Arch. f. d. ges. Physiol. **103**:171, 1904.

tions so arranged that the circulation in the head and forelegs of an animal was maintained by a heart-lung preparation and that in the hind legs by means of a preparation of its own heart and lungs, Anrep and Starling[22] (1924) were able to determine that decrease in flow (or in pressure) within the vasoconstrictor center caused a rise in the general systemic arterial blood pressure and that an increase in flow (or in pressure) caused a drop in the general pressure. They further concluded from their experiments that afferent impulses through the vagus (as in high pressure within the aorta) caused arterial dilatation and increased flow in the head end of the preparation. Epinephrine raised the pressure in the head end of the animal and lowered the general blood pressure. The authors interpret these results as not indicating any cerebral vasoconstriction. Gesell and Bronk[74] (1926), using a continuous thermo-electric method of their own to record volume flow of blood, found an inverse relation of carotid and of femoral flows in response to a given stimulation (high carbon dioxide or low oxygen). This suggested that, even when the mean blood pressure was kept nearly constant by artificial means, circulatory adjustments occurred in favor of the brain. It is suggested that the adjustments were due, partly at least, to a central regulating mechanism.

3. *Histologic Studies.*—Gulland[2] (1895), using the methylene blue staining method, could not find nerves on the cerebral vessels. Obersteiner[75] (1897), using the gold chloride impregnation method, said that he found what he believed to be nerve fibers on the pial and other cerebral vessels. Gulland[76] (1897) repeated his earlier work and retracted his earlier statement. He described nerve fibers on the cerebral vessels. Morison[77] (1898), using Sihler's hematoxylin, believed that he found nerve fibers. Huber[78] (1899) injected a 1 per cent methylene blue solution into the carotid artery and by this means demonstrated nerve fibers on the pial arteries. Hunter[79] (1900), using similar methods, confirmed Huber's work but asserted that the nerve fibers did not go any further than the gray matter and did not extend into the white. Ford Robertson[80] (1899) used the platinum impregnation method and found strands on the cerebral blood vessels. He was not convinced that they were nerves and thought they might be connective tissue. Rohnstein[81] (1900) found similar strands and believed with Robertson that they were connective tissue. Philip Stohr[82] (1922) carefully reviewed the subject, using modern methods of impregnation with silver (modification of the Bielschowsky method). He was convinced that cerebral vessels have nerves and suggested that vasomotor fibers may have many other routes than along the cervical sympathetic nerves. Orr and Sturrock[83] (1922) studied the brain histologically, following section of the cervical sympathetic nerve on one side, and described a homolateral dilatation of the cerebral vessels. They believed that the influence of the sympathetic was unilateral.

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74. Gesell, R., and Bronk, D. V.: Proc. Soc. Exper. Biol. & Med. **24**:255, 1926.
75. Obersteiner, O.: Arb. r. d. Inst. f. Anat. u. Physiol. des Centralnervensystems **5**:215, 1897.
76. Gulland, G. L.: Rep. Lab. Coll. Phys., Edinburgh **6**:55, 1897.
77. Morison, A.: Edinburgh M. J. **4**:413, 1898.
78. Footnote 8, third reference.
79. Hunter, W.: J. Physiol. **26**:465, 1900-1901.
80. Robertson, W. F.: Scot. M. & S. J. **4**:23, 1899.
81. Rohnstein, R.: Arch. d. mikr. Anat. **55**:576, 1900.
82. Footnote 6, sixth reference.
83. Orr, D., and Sturrock, A. C.: Lancet **203**:267, 1922.

4. *Perfusion Methods.*—Ferrier and Brodie[84] (1902) concluded that epinephrine can constrict cerebral blood vessels, although a large dose of the extract is required. Wiggers[85] (1905), using perfusion methods, concluded that epinephrine can cause cerebral vasoconstriction, but that it is readily overcome by elevation of the systemic arterial blood pressure. Later (1908)[85], again using perfusion methods, he obtained evidence of vasoconstriction on stimulation of the sympathetic fibers about the internal carotid artery near the base of the brain and concluded that there were vasoconstrictor nerve fibers to the cerebral arteries. Dixon and Halliburton[86] (1910) concluded, after perfusion experiments, that epinephrine causes cerebral vessels to dilate slightly. Gruber and Roberts[87] (1926), using epinephrine in a solution so neutralized that the hydrogen ion concentration equalled that of normal blood, observed that strong concentrations of epinephrine caused cerebral vasoconstriction in perfused brains. They explained the inconsistency in previous experiments in perfusion as the result of failure to recognize the importance of the perfusion fluid reaction.

5. *Measurements of Temperature.*—Claude Bernard[88] (1858) noted that the temperature of the brain on the side on which the superior sympathetic ganglion had been extirpated was higher than on the opposite normal side.

6. *Study of the Surviving Artery.*—Dixon and Halliburton[86] (1910) used rings from various parts of the cerebral arteries and immersed them directly in solutions of epinephrine. Constriction was not observed. Cow[89] (1911), using a similar method, saw actual dilatation of the cerebral artery. Mention of the *p*H of the solution is not made.

7. *Use of the Systemic Changes Produced as an Indicator of Cerebral Vasoconstriction.*—Kussmaul and Tenner[88] (1857) produced fits in one of three rabbits by tying off the carotid on one side and stimulating the sympathetic nerve of the neck on the other. This led them to conclude that vasoconstriction occurred on the stimulated side. Roberts[90] (1921) believed that the apnea and Cheyne-Stokes breathing that occurs in anesthetized cats and rabbits when epinephrine is given intravenously is the result of cerebral anemia from vasoconstriction.

8. *Studies Made by Means of Defects of the Bone in Human Craniums.*—Raphael and Stanton[91] (1919) noted through defects of the bone in a human cranium that epinephrine given intravenously at first caused reduction and then increase in the volume of the brain.

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84. Ferrier and Brodie, T. G., quoted from Brodie, T. G., and Dixon, W. E.: J. Physiol. **30**:476, 1904.

85. Wiggers, C. J.: Am. J. Physiol. **20**:206, 1907-1908; **21**:454, 1908, also footnote 8, first reference.

86. Dixon, W. E., and Halliburton, W. D.: Quart. J. Exper. Physiol. **3**:315, 1910; J. Physiol. **48**:128, 1914.

87. Gruber, C. M., and Roberts, S. J.: J. Pharmacol. & Exper. Therap. **27**:335, 1926.

88. Footnote 7, first reference.

89. Cow, D.: J. Physiol. **42**:125, 1911.

90. Roberts, F.: J. Physiol. **57**:405, 1923.

91. Raphael, T., and Stanton, J. M.: Action of Certain Drugs on Brain Circulation in Man, Arch. Neurol. & Psychiat. **2**:389 (Oct.) 1919.

NEURINOMA

A CASE OF INVOLVEMENT OF THE CAUDA EQUINA WITH THE CLINICAL PICTURE OF BILATERAL SCIATICA *

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A neurinoma originates in the embryonic cells of Schwann, or, as some put it, in the precursors of these cell elements. Antoni [1] calls them lemmoblasts. Described mainly in the posterior roots of the spinal cord, neurinomas also occur in: the pharynx and bronchi (Askanazy [2]); the gastro-intestinal canal (Orzechowski and Nowicki,[3] Banerjee and Christeller [4]); the cerebral nerves, including the optic (Reverdin and Grumbach [5]) but especially in the acoustic, and even in the central nervous system—the brain (Josephy [6]) and spinal cord (Kirch [7]). This type of tumor is not rare, for Antoni found it in twenty of thirty cases of tumors of the spinal cord that he studied, while Sommer [8] gave an account of thirty-seven and Erb [9] of ten additional cases; to these may be added eight cases studied by Borchardt.[10] In all, fifty-five cases of neurinomas have been recorded.

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* Read at a Meeting of the Chicago Neurological Society, Oct. 20, 1927.

1. Antoni, N.: Ueber Rückenmarkstumoren und Neurofibroma, Munich, J. E. Bergman, 1920.

2. Askanazy, M.: Ueber schwar erkennbare Neurofibromatose, Centralbl. f. d. allg. Pathol. u. pathol. Anat. **24**:961, 1913; abstracted in ibid. **25**:875, 1914.

3. Orzechowski, K., and Nowicki, W.: Zur Pathogenese und pathologischen Anatomie der multiplen Neurofibromatose und der Sclerosis tuberosa, Ztschr. f. d. ges. Neurol. u. Psychiat. (Orig.) **11**:237, 1912.

4. Banerjee, N. S., and Christeller, E.: Ueber die gastrointestinale und andere Lokalization der Neurofibromatosis (Morbus v. Recklinghauseni), Virchows Arch. f. path. Anat. **261**:50, 1926.

5. Reverdin, A., and Grumbach: Un cas de neurinome du nerf optique, Ann. d'anat. pathol. médico-chirurgicale **2**:229 (May) 1925.

6. Josephy, H.: Ein Fall von Porobulie und solitärem, zentralen, Neurom, Ztschr. f. d. ges. Neurol. u. Psychiat. **93**:62, 1924.

7. Kirch, E.: Zur Kenntniss der Neurinome bei Recklinghausenscher Krankheit, Ztschr. f. d. ges. Neurol. u. Psychiat. **74**:379, 1922.

8. Sommer, R.: Der heutige Stand der Neuromfrage, Bruns' Beiträge zur klin. Chir. **125**:694, 1922.

9. Erb, Karl: Zur Neurinomfrage, Deutsche Ztschr. f. Chir. **181**:350, 1923.

10. Borchardt, M.: Zur Kenntniss der Neurinome, Bruns' Beiträge z. klin. Chir. **138**:1, 1927.

In former years, this type of tumor used to be considered as a neuroma, a "pure nerve" tumor, consisting of hypertrophied parenchymatous nerve tissue (true neuroma of Virchow [11]). Recklinghausen,[12] in 1882, showed that the majority of so-called neuromas are made up of connective tissue (false neuromas of Virchow) and classified them as fibromas of the nerves (neurofibromas). In 1908 and again in 1910, Verocay [13] showed that in his two cases the multiple tumors (neurofibromas) were not composed of connective tissue; that is to say they were not mesodermogenic but were ectodermogenic tumors, the product of abnormal growth and proliferation of the embryonic cells of Schwann. He adopted the name neurinoma. Like neurofibromas, neurinomas may be solitary or multiple and exceedingly numerous. Wallner [14] termed the condition of multiple tumors neurinomatosis, in contrast to neurofibromatosis (Recklinghausen's disease). These types of multiple tumors of the nerve are mainly of academic interest, for they are inaccessible to treatment. Solitary neurinomas (or neurofibromas), in contrast, are of great practical importance. Like endotheliomas, neurofibromas, sarcomas, "gliosarcomas," "fibrogliosarcomas" and numerous other tumor combinations, neurinomas may resemble highly malignant tumors, and may suggest, therefore, an unfavorable prognosis; they may consequently be the object of undesirable surgical procedures. It is therefore imperative to differentiate neurinomas from tumors that resemble them, to diagnose them early and to effect a radical cure by timely and proper surgical procedures.

Aside from the type of the tumor, I wish to point out several other features of interest in the case to be reported—the young age of the tumor, its localization in the cauda equina and the rather unusual clinical picture.

REPORT OF CASE

Clinical History.—A white woman, aged 46, entered the neurologic service of the Cook County Hospital on Jan. 9, 1927, because of pain in both lower extremities. This had begun, about three months prior to admission to the hospital, in the joint of the left hip. It was sharp and constant, and radiated along the thigh and leg to the foot. The pain would become exaggerated on attempts at urination or bowel movement, or when the patient adopted a recumbent

11. Virchow, R.: Die krankhaften Geschwülste, **3**:242, 1865; Das wahre Neurom, Virchows Arch. f. path. Anat. **13**:256, 1858.

12. Recklinghausen, F.: Ueber die multiple Fibrome der Haut und ihre Beziehung zu den multiplen Neuromen, Berlin, A. Hirschwald, 1882.

13. Verocay, Jose: Multiple Geschwülste als Systemerkrankung am nervösen Apparate, Festschrift Prof. Hans Chiari, Wien, B. Braumüller, 1908; pp. 378-415; Zur Kenntniss der Neurofibrome, Beitr. z. path. Anat. u. z. allg. Pathol. **48**:1, 1910.

14. Wallner, A.: Beitrag zur Kenntniss des Neurinoma Verocay, Virchows Arch. f. path. Anat. **237**:331, 1922.

posture; an upright position or walking would relieve it. For this reason the patient spent many sleepless nights walking the floor and also refrained from moving the bowels. Eleven weeks after the onset of pain in the lower left extremity, a similar pain commenced on the right side, but was here less severe and caused the patient a negligible amount of discomfort. In addition to the pain in the extremities, the patient suffered much from headache and nocturia (urinated seven times during the night).

Vaginal hysterectomy for a cystoma had been performed at the Cook County Hospital in March, 1925.

Examination.—Puffiness of the lower eyelids was present; cutaneous lesions, such as tumors, abnormal pigmentation, deformities and muscular atrophies were absent. Some tenderness was present over both great trochanters and the posterior aspects of both thighs, along the course of the sciatic nerves. The Lasègue sign was positive, especially on the left, while the so-called hip sign of Patrick was negative, that is to say, the flexed leg with the corresponding heel or external malleolus placed over the opposite knee could be brought down to the level of the bed without causing the patient any pain or discomfort. Muscle power was good throughout; coordination, muscle tonus and gait were normal. The tendon reflexes were lively throughout, without a Babinski sign or clonus. Sensory disturbances of all kinds were absent, except for hyperesthesia of both legs and feet to pressure, pinpricks and even to touch.

The pupils reacted sluggishly to light but well in accommodation. The cranial nerves and mental condition were normal.

Examination of the heart revealed the apex beat in the sixth interspace, a little to the left of the midclavicular line. The tones were clear, but the second pulmonic sound was accentuated. The lungs were normal.

Vaginal examination revealed adhesions binding down the pelvic tissues, and absence of the pelvic organs; rectal examination gave negative results.

The blood pressure was 220 systolic and 0 diastolic; the pulse was of high tension. The Wassermann reaction with the blood and the spinal fluid was negative. Blood chemistry studies showed: nonprotein nitrogen, 39 instead of 26; urea nitrogen, 18.68 instead of 10; uric acid 2.25, and creatinine, 1.50. Examination of the urine revealed albumin (+++) and a moderate number of hyaline, coarse granular and epithelial casts.

Roentgen-ray examinations of the lumbar spine and of the pelvic region showed an "arthritic sacro-iliac sclerosis" and a defect at the level of the first sacral segment, which though interpreted as spina bifida occulta was considered by the roentgen-ray department as of no diagnostic value.

Summary of Clinical Observations and Diagnostic Considerations.—The history of pain, which was first unilateral and later bilateral, suggested a lesion of the cauda equina. In the absence of other signs or symptoms it was not possible to determine the nature of the lesion. The history of a pelvic operation suggested the possibility of a metastasis. This, however, had to be rejected after the nature of the operation had been ascertained. The fact that the patient was more comfortable when up and around and experienced more pain when on her back or on attempts at defecation suggested a pressure on the cauda equina, probably by a tumor. Arrangements were accordingly made for an operation, but before the patient was transferred to the surgical ward on March 13, 1927, she became suddenly stuporous and paralyzed on the left side. The blood pressure was 220 systolic and 140 diastolic; the spinal fluid was bloody and a Babinski sign appeared on the left. Death followed on March 16, 1927.

Necropsy Observations (Dr. Schmitt).—Necropsy revealed a right internal capsule hemorrhage which had broken through the caudate nucleus into the lateral ventricles; "neurofibroma" of the cauda equina; atheromatous degeneration and calcification of the aorta; atherosclerosis; arteriosclerotic scarring of the kidneys; bilateral focal bronchopneumonia; total hysterectomy; bilateral salpingectomy and left oöphorectomy; chronic glomerulonephritis.

Fig. 1.—Tumor (*T*) 8 inches below the conus medullaris adherent to one of the sacral roots and covered by the rest of the cauda equina.

Macroscopic Examination.—The tumor (fig. 1) measured ½ by ¾ inch (1.2 by 1.9 cm.); it was adherent to the second posterior sacral root on the left; approximately 8 inches below the conus medullaris. The tumor was smooth, rather soft to touch, possessed a capsule and was covered by, but not adherent to, the rest of the roots of the cauda equina. No other tumors, thickenings, adhesions or hemorrhages, were found over the cauda equina or elsewhere. In short, a solitary tumor of the cauda equina was present and adherent to one of its roots.

Microscopic Examination.—Histologic study showed (fig. 2) an abundance of nuclei, embedded within a homogeneous substance which stained yellowish with the method of van Gieson. The nuclei of the tumor formed parallel rows or in some instances, as figure 2 shows, concentric layers or whorls; in other areas oblong bands of nuclei crossed and recrossed one another. Under higher power the nuclei appeared roundish, rodlike, spindle shaped, rich in chromatin, and without visible processes or cell bodies. In rather rare instances the nuclei were arranged in palisade form; this was not marked. Fig. 2 shows (at *X*) such an arrangement of nuclei with the formation of somewhat denser rows. Around some capillaries and blood vessels (fig. 3) the nuclei were much

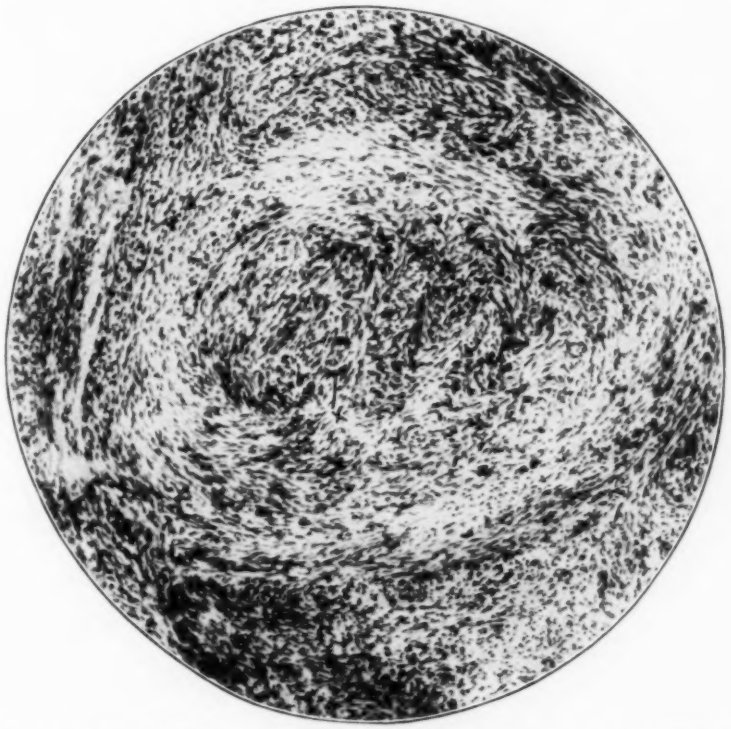

Fig. 2.—Microscopic section of the tumor, showing, in the middle of the picture, the whorls surrounded by parallel rows of nuclei. At *X* and in the neighboring area a palisade arrangement can be made out. Connective tissue or other fibers and fibrils are absent. The small dots scattered in pale areas are transversely cut nuclei; van Gieson, × 100.

denser. Blood vessels were abundant, their walls well developed and without relationship to the capsule (fig. 4). The capsule consisted of connective tissue; like the blood vessels it stained bright red, contained within its meshes numerous nuclei of the tumor mass and did not give any prolongations into its interior, which, as said, did not contain connective tissue formations other than the blood vessels. A small portion of the tumor was represented by a cystic hyaline mass (fig. 4*h*). Fibrils were not seen in the nuclear mass with the methods of Alzheimer-Mann, Bielschowsky or Holzer. Even with the

method of Perdrau distinct fibrils could not be demonstrated, so that one must admit that the internuclear substance was structureless and homogeneous.

The spinal cord, stained with the method of Weigert-Pal, did not show any changes, such as secondary degeneration; nor were any found in the rest of the cauda equina or in the spinal membranes. The tumor was solitary, resembling in its structure similar tumors described by Rheinberger,[15] Borchardt, Derman [16] and others.

COMMENT

The outstanding clinical observations were: severe pain of three months' duration, first in one leg then in the other, relieved by an

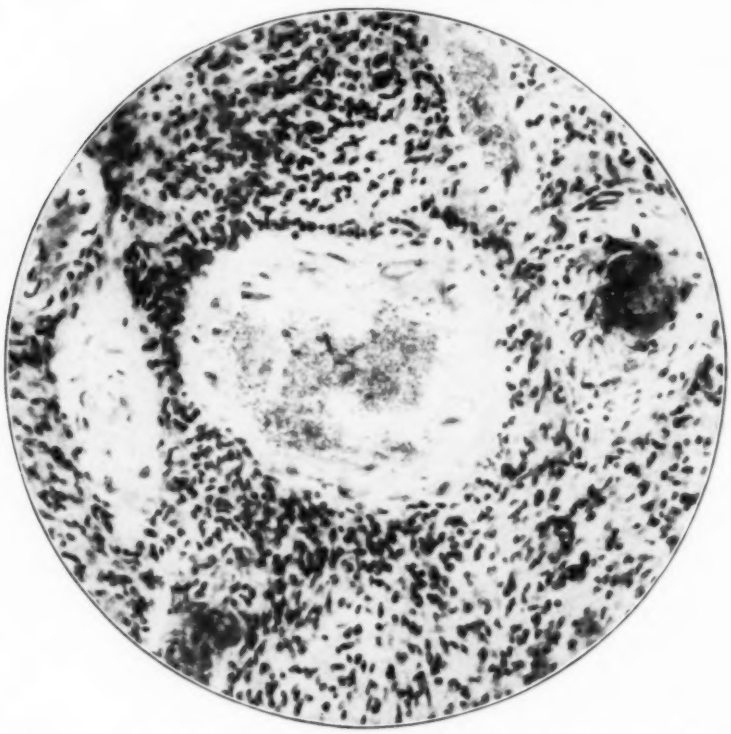

Fig. 3.—Accumulation of cells around a vein; below and to the left this is especially dense; hematoxylin and eosin, × 230.

upright and aggravated by a recumbent posture; extreme sensitiveness to touch over the second and third sacral root areas; nocturia, and lack of manifest sensory and motor disturbances. Roughly, they resembled

15. Rheinberger, M.: Ueber einen eigenartigen Rückenmarkstumor vom Typus des Verocayschen Neurinoms, Frankf. Ztschr. f. Path. **21**:472, 1918.

16. Derman, G. L.: Ein Beitrag zur Kenntniss der Neurinomatose, Centralbl. f. allg. Path. u. path. Anat. **37**:52, 1926; Zur Kenntniss der Kleinhirnbrückenwikelneurome, Virchows Arch. f. path. Anat. **261**:39, 1926.

what is generally spoken of as bilateral sciatica. If great care is to be exercised in diagnosing unilateral sciatica, however, still greater caution should be taken when the pain is along both sciatic nerves. Bilateral sciatic pain should always suggests an organic lesion of the cauda equina or its membranes. Tabes, tumors, syphilitic or carcinomatous meningeal infiltrations, pelvic disorders affecting the sacral plexus, constitutional or toxic states—all may give a picture of bilateral sciatica. It may be produced even by functional nerve disorders, such as neurasthenia and hysteria, which should be diagnosed only when the foregoing morbid

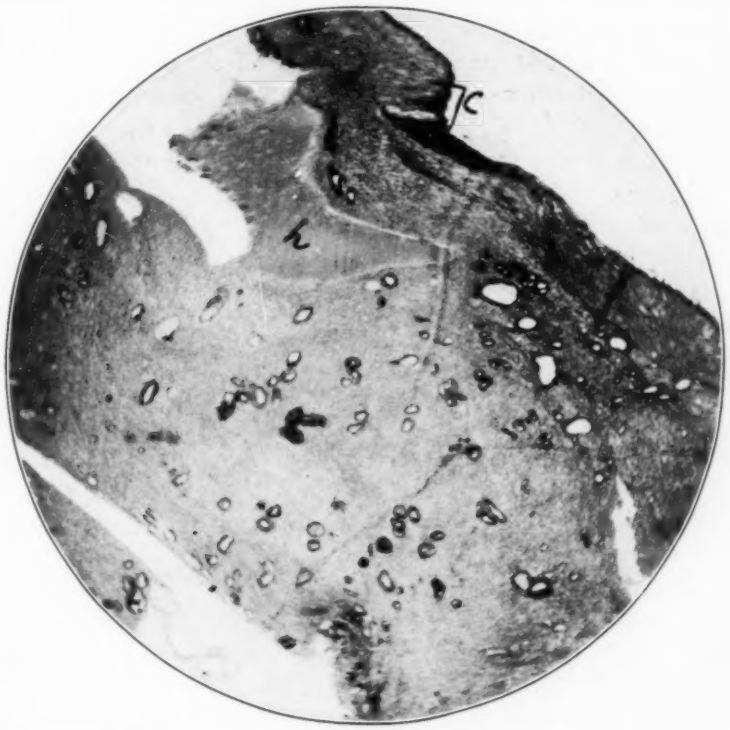

Fig. 4.—The field is covered with numerous blood vessels—round empty spaces surrounded by dark, stained rings (adventitial layers). At *C* above is the capsule of the tumor and at *h* a hyaline cystic mass. The rest of the field is devoid of any fibrillary structure, appearing somewhat homogeneous, and under higher power showing contours of nuclei; Alzheimer-Mann, × 38.

states can be safely excluded. In the case here recorded the cause of the bilateral sciatic pain was a tumor which evidently had not been there long enough to cause sensory or motor disturbances.

The location of the tumor explained well the preservation of the knee jerks; the nocturia was probably due to pressure of the tumor on the lower sacral roots; the relief from pain in an upright position

was due to changes in the mechanical relationship and consequent displacement of the tumor, while the extreme hypersensitiveness was rather an unusual feature, as it commonly occurs in inflammations of the peripheral nerve (polyneuritis). The ordinary signs and symptoms of a cauda equina lesion—scattered atrophy, asymmetrical sensory disturbances, loss of tendon reflexes especially of the tendo achillis, bladder and rectal disturbances—were not present in this case. The foregoing signs occur commonly in states of long standing;[17] their presence makes the diagnosis easy. In early cases they are mostly lacking, the entire clinical picture being represented by pain. In the case of Raymond,[18] it was the only symptom for a period of eight years.

Of equal interest is the pathologic phase of the case here recorded. The type of the tumor cells, or rather nuclei, their arrangement in parallel rows, their staining qualities, the localization of the tumor, all justified the diagnosis of neurinoma. Such a tumor of the caudal portion of the spinal cord is usually a partial manifestation of a generalized neurofibromatosis or neurinomatosis, in which the tumors are scattered in great numbers over the cerebral, spinal, cutaneous and sympathetic nerve fibers. A solitary tumor in the cauda equina is rare, only two cases having been mentioned by Antoni [1] in his series. Careful study of tumors in Recklinghausen's disease (general and central neurofibromatosis) and general neurinomatosis when they are of various sizes and in various stages of growth—young, old, small and large—showed them to consist of a peculiar tissue. This has been described by Verocay, Antoni and their followers as "nuclear bands" scattered over "fine fibrillary bundles" that much resembled nerve or glia tissue. Verocay, however, would not identify neurinoma with either of the latter.

He held that neurinomas are made up of tissues *sui generis*, and originate in the so-called "nerve fiber cells" or embryonal cells of Schwann, which were termed by Antoni lemmoblasts. As Schwann cells are considered peripheral glia cells (Held), Lhermitte and Leroux [19] termed these tumors peripheral gliomas. Some features typical of a neurinoma are well shown in figures 2 and 3—abundance of nuclei arranged in parallel rows, with the formation of whorls and the dense accumulation of nuclei around some blood vessels, and with indications of a palisade arrangement of the nuclei.

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17. Hassin, G. B.; Johnstone, K., and Carr, A.: Bullet Lesion of Cauda Equina, J. A. M. A. **66**:1001 (April 1) 1916.

18. Raymond, F.: Sur quelques affections de la queue de cheval, Arch. gen. de méd. **97**:1940, 1906.

19. Lhermitte and Leroux: Étude histologique générale des gliomes des nerfs périphériques des racines rachidiennes et des gliomes visceraux, Rev. neurol. **39**:286, 1923.

However, the histologic picture of neurinoma is not uniform. In cases in which the tumors are multiple, some are found to be exceedingly nuclear or cellular and resemble sarcomas. Here the connective or fibrillary tissue is meager, being confined to the capsule or to the blood vessels. Other tumors, in contrast, are poor in cells but rich in connective tissue, showing, therefore, as fibromas. In other tumors again, the two types may be combined to form mixed tumors, which have been described as neurinofibromas. Antoni classifies the neurinomas as fibrillary (type *A*) and areolar (type *B*). In the former, the nuclei are

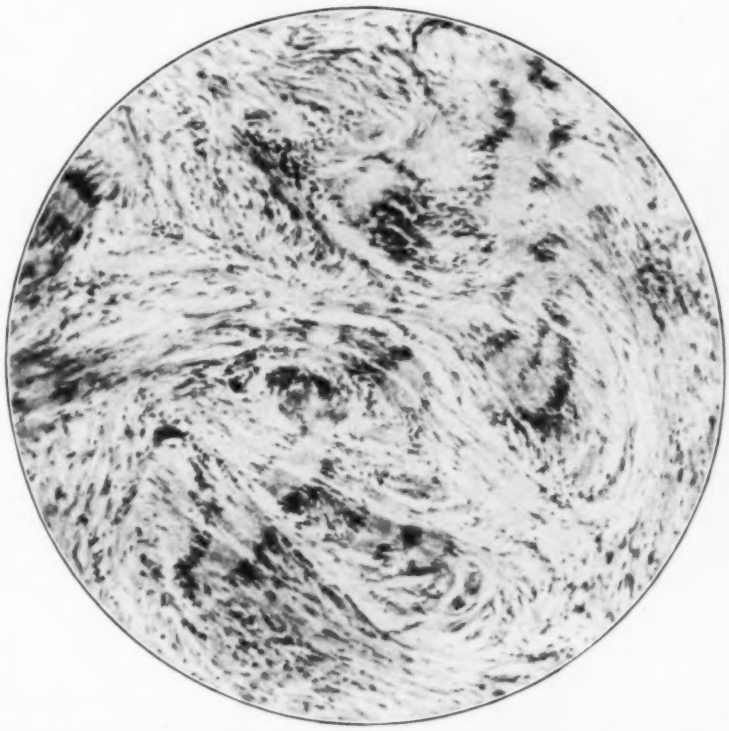

Fig. 5.—Neurinoma of the face (a case of Dr. H. R. Jaffé); the palisade arrangement (dark rows of cells) is distinct; Spielmeyer's hematoxylin, × 100.

parallel to the fibrils; their wealth and the vivid whorl arrangement give them the aspect of a fibrosarcoma or an endothelioma. In type *B* the nuclei are much less in evidence, while in the mixed type the fibrils are prominent at the periphery and less so in the center where the nuclei predominate. Because of lack of uniformity in structure, the histologic diagnosis of neurinoma often presents great difficulties; it may, as in this case, resemble a sarcoma, an endothelioma or a glioma. Judging from the history and the clinical course in my case the tumor was not much older than six months, that is to say, it was not an old tumor, the

young age being probably responsible for the predominance of the nuclei. Had it been older, it might have shown an abundance of connective tissue, poverty in nuclei and cells and thus have resembled a fibroma. Erb states that the "pure form of neurinoma" shows in young tumors, and that in comparing small and large tumors one gains an impression that eventually the stroma will replace or substitute the tumor tissue proper. In short, the apparent polymorphism in the structure of neurinomas is most likely due to the age, a fact already pointed out by Wallner.

Aside from the age, the localization of the tumor may influence its structure. A neurinoma of the acoustic nerve, for instance, will give a structure different from one on a peripheral nerve or root, or from a

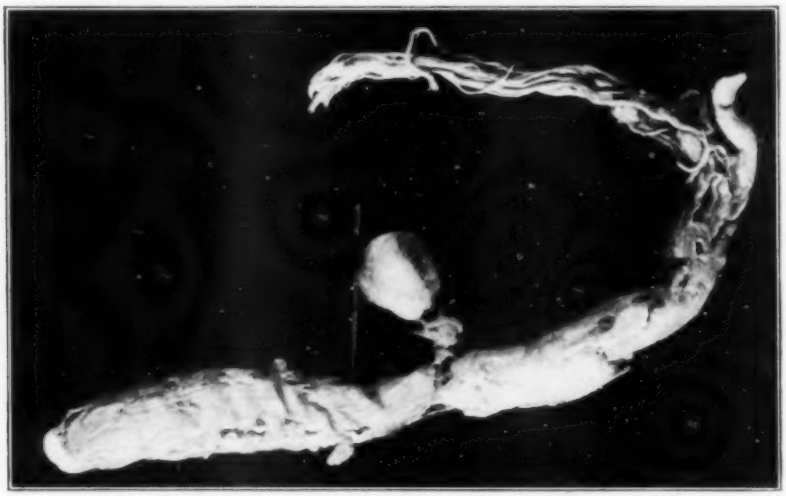

Fig. 6.—Recklinghausen's disease—tumor in connection with the spinal meninges. The connective tissue fibers form massive bundles enclosing large foci of nuclei; van Gieson, × 100.

Recklinghausen's neurofibroma. Figure 5, for instance, represents the structure of a peripheral nerve (facial) neurinoma with the characteristic palisade cell arrangement; the nuclei are fewer than in the case here recorded, but connective tissue is more abundant. Figure 6 shows a tumor of the spinal meninges in a case of Recklinghausen's disease of many years' duration. The deep layers of this tumor show abundance of connective tissue, arranged in bundles of collagen fibers forming meshes enclosing nuclei, small and round in shape and scattered irregularly, that is to say, without formation of parallel rows. The meshes resemble the areolar type *B* of Antoni a great deal. The peripheral portions of the tumor were poor in nuclei but rich in connective tissue fibers, among which ganglion cells were common.

The foregoing two types of tumors, together with the one under discussion, may be considered as belonging to one class; that is to say, all

are probably neurinomas, the difference in structure being due to the age, localization and resultant reactive phenomena. These were much stronger in the peripheral neurinomas and in the Recklinghausen type than in the cauda equina tumor, and the tumors themselves were much older. The cellular, or rather nuclear, neurinoma (fig. 2) here described certainly differs greatly from the so-called neurofibroma of Recklinghausen. Some investigators (Sommer, Krahn [20]) therefore consider these two tumors as distinct pathologic entities. Other

Fig. 7.—The macroscopic appearance of the tumor pictured in figure 6.

pathologists (Harbitz,[21] Krumbein [22]) do not recognize neurinoma as a specific tumor; they refer all multiple tumors to Recklinghausen's

20. Krahn, Hans: Untersuchungen an Neurinomen, Centralbl. f. allg. Pathol. u. path. Anat. **38**:113, 1926.

21. Harbitz, F.: Multiple Neurofibromatosis (v. Recklinghausen's dis.), Arch. Int. Med. **3**:32 (Feb.) 1909; Beitr. z. path. Anat. u. z. allg. Pathol. **62**:503, 1916.

22. Krumbein, C.: Ueber die Band- oder Pallisadenstellung der Kerne, eine Wuchsform des feinfibrillären mesenchymalen Gewebes, Virchows Arch. f. path. Anat. **255**:309, 1925.

disease, while Mallory,[23] and after him Penfield,[24] define a "solitary encapsulated tumor which is found attached to spinal nerve roots (as in the case here recorded), cranial nerves and peripheral nerves as "perineural fibroblastoma," i. e., as connective tissue tumors. It should be mentioned here that Herxheimer and Roth [25] long ago emphasized, on different grounds, that connective tissue plays a more considerable rôle in the genesis of neurinoma than was originally ascribed to it by Verocay. The majority are inclined to look on neurinoma, neurofibroma and central neurofibromatosis, in which the tumors are located at the base of the cranial nerves or spinal roots (described by Henneberg and Koch,[26] Bassoe and Nuzum,[27] Ayala and Sabatucci [28]), as one disease process—neurinomatosis. The genesis of the tumors in each of these conditions seems to favor the latter view.

The practical interest of neurinomas is great when the tumors are solitary and therefore accessible to surgical treatment. They are often mistaken for malignant tumors such as sarcomas and their various combinations. The description given for some such tumors does not leave any doubt as to their true nature. In a woman, aged 70, Luschka [29] found three tumors on the roots of the cauda equina (third and fourth lumbar bilaterally) and on both root fibers of the coccygeal nerve. Microscopically, they showed oblong rounded nuclei and spindle-shaped cells, arranged within fibrillary bands. To Luschka it was an "undoubted" sarcoma, essentially identical with what is called "Faser-Kern-Geschwulst." In the same year (1857), Benjamin [30] described a solitary tumor of the cauda equina in a man, aged 60, who had suffered for seven years from pain in the lower extremities. It was made up of soft parts that contained "pus" cells, and solid "points" made up of

23. Mallory, F. B.: The Type of Cell of the So-Called Dural Endothelioma, J. M. Research **41**:349 (March) 1920.

24. Penfield, W.: The Encapsulated Tumors of the Nervous System, Surg. Gynec. Obst. **45**:178 (Aug.) 1927.

25. Herxheimer, G., and Roth, W.: Zum Studium der Recklinghausenschen Neurofibromatose, Beitr. z. path. Anat. u. z. allg. Pathol. **58**:320, 1914.

26. Henneberg and Koch, M.: Ueber "centrale" Neurofibromatose und die Geschwülste des Kleinhirnbrückenwinkels (Acusticusneurome), Arch. f. Psychiat. **36**:251, 1902.

27. Bassoe, P., and Nuzum, F.: Report of a Case of Central and Peripheral Neurofibromatosis, J. Nerv. & Ment. Dis. **42**:785 (Dec.) 1915.

28. Ayala, G., and Sabatucci, F.: Klinischer und pathologischanatomischer Beitrag zum Studium der zentralen Neurofibromatose, Ztschr. f. d. ges. Neurol. u. Psychiat. **103**:496, 1926.

29. Luschka, H.: Die Faser-Kern-Geschwulst an Wurzeln von Rückenmarksnerven, Virchows Arch. f. path. Anat. **11**:384, 1857

30. Benjamin, L.: Neurom innerhalb der Rückenmarkshäute, Virchows Arch. f. path. Anat. **11**:87, 1857.

"fibrils." The diagnosis was neuroma. Inglis, Klingman and Ballin [31] successfully removed an extraspinal tumor which they diagnosed as a "true glioma (extramedullary)" that originated in the "neuroglia sheath of the seventh dorsal nerve." With such an origin of the tumor it is probable that it was a neurinoma. Moritz [32] speaks of two small tumors of the cauda equina and two of the conus medullaris in a patient who had suffered from bilateral sciatic pain. He does not give a microscopic description of the tumors which were diagnosed as "angiosarcoma telangiectodes hemorrhagicum" and originated in external "Wandschichten."

Joseph Hertle [33] found a tumor at the level of the fourth lumbar root in a patient who for eight years had been treated for sciatica. The tumor consisted of spindle shaped cells forming parallel rows, "glia tissue" with extreme wealth of cells and "transitions" into a fibrous form. The diagnosis was neurinofibroglioma. The patient recovered.

In the case of Hippel,[34] cauda equina tumors were combined with others in the rest of the central nervous system. They were diagnosed as spindle cell sarcomas, but from the description were most likely neurinomas.

Of great interest are the tumors in the peripheral nerves where they may be multiple, as in the case of Keen and Spiller.[35] Single tumors diagnosed as malignant were reported, among others, by McArthur,[36] Oehlcker,[37] McGuire and Burden, Jr.[38] In some of these (Oehlcker's, for instance) cases complete recovery set in, in spite of the diagnosis of sarcoma. Probably they were neurinomas, which are benign tumors of rather favorable prognosis and therefore do not require radical surgical procedures such as amputation. This operation has been performed repeatedly in such cases.

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32. Moritz: Ein Fall von Angio-Sarcom der Cauda Equina, St. Petersb. med. Wchnschr. **37**:174, 1912.

33. Hertle, J.: Tumor d. Cauda Equina durch Operation entfernt. Bildung einer Liquorcyste an der Operationstelle. Neuerliche Operation. Heilung, Arch. f. Psychiat. **59**:861, 1918.

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35. Kreen, W. W., and Spiller, W. G.: A Case of Multiple Neurofibromata of the Ulnar Nerve, Am. J. Med. Sc. **119**:526, 1900.

36. McArthur, L. L.: A Sarcoma of the Posterior Tibial Nerve; Excision; Removal of Metastatic Foci in Retroperitoneal Lymph Glands Three Months Later, Surg. Clinics Chicago **41**:130 (Feb.) 1920.

37. Oehlcker, F.: Ueber Neurofibrome des N. Tibilis, Deutsche Ztschr. f. Nervenh. **68-69**:211, 1921.

38. McGuire, and Burden, Jr.: Unusual Case of Sarcoma of the Median Nerve, Surg. Gynec. Obst. **35**:453, 1922.

CONCLUSIONS

1. For a long time a tumor of the cauda equina may not show symptoms other than pain along both sciatic nerves with marked hyperesthesia.

2. It may be a partial manifestation of so-called Recklinghausen's disease, central neurofibromatosis or general neurinomatosis.

3. These three morbid conditions are most likely manifestations of one disease process—neurinomatosis.

4. The tumor may be solitary and its structure polymorphous, the polymorphism being due to the age of the tumor and its localization.

5. Though resembling malignant tumors (such as sarcomas and gliomas), neurinomas are benign neoplasms.

ABSTRACT OF DISCUSSION

DR. PETER BASSOE: Does Dr. Hassin believe that sarcoma ever develops in these so-called neurinomas? I have seen a statement that it does in about 10 per cent.

DR. HASSIN: Krumbein mentions a case of spindle cell sarcoma that much resembled a neurinoma. The resemblance is sometimes so great that a mistaken diagnosis is quite pardonable. One even finds in the literature such terms as neurinoma sarcomatoides. However, one cannot speak of a neurinoma being transformed into a sarcoma, for the former is supposed to be an ectodermogenic and the latter a mesodermogenic tumor.

Abstracts from Current Literature

ON THE UNDERSTANDING OF RECURRENT OPHTHALMOPLEGIA. CORNELIA DE LANGE, Deutsche Ztschr. f. Nervenh. **96**:225 (March) 1927.

Recurrent ophthalmoplegia affects children and younger persons more often than adults. Many authors agree that those cases that show ptosis only belong also in this group. It is nearly always associated with headaches, in the form of migraine and vomiting. It never alternates but affects always the same oculomotor nerve.

As to its cause, some authors are inclined to consider it a disease of the oculomotor nuclei; others consider it a disease of the roots as well as of the nuclei; still others consider it a disturbance of the base. In the cases in which a necropsy was performed the oculomotor stem was found involved. The periodicity of the disease is said to be due to a vasomotor disturbance. De Lange reports three cases of recurrent ophthalmoplegia differing somewhat in their symptomatology from the general type, with a complete postmortem examination in one case.

Case 1 was that of a girl, aged 6, with a pericardial and pleural exudate. Later, she developed myocarditis with endocarditis and pericarditis. A cerebral embolus resulted in a right hemiplegia. A few days after she entered the hospital left ptosis developed. The pupils were equal and reacted to light and in convergence. The ptosis was variable; at times it was extremely marked and again it was mild or would entirely disappear. Such variations could be noticed sometimes even during the same day. The parents stated that they had noticed the recurrent affection of the eye ever since the child was born. There was no headache nor vomiting with the attack. The few days before the patient died the ptosis remained constant, although it varied in severity. A postmortem examination was not performed. The author, therefore, can only speculate on the etiology in this case. If the parent's statement regarding the recurrent ptosis since birth can be accepted, it must be assumed that there was a congenital disturbance of the oculomotor nucleus, an ambiotrophy in the sense of Gowers, and the remissions can be explained on the periodic improvement of the pathologic process. The absence of headache and vomiting would go well with this conception. Another possibility is that there was a basal involvement; the child showed a positive Pirquet reaction. The pathologic process would fit in with such a symptomatology.

Case 2 was that of a boy, aged 8, who two years since he entered school had had frequent attacks of nausea and vomiting, during which time the left eye became smaller. He did not have any headache. The attacks occurred more often on Monday morning at the beginning of a school week. On examination, De Lange found a mild ptosis and no other neurologic disturbance. The patient recovered under psychotherapy. The author properly considers this a case of functional disturbance.

Case 3 was that of a woman, aged 60. When she was 28, she suffered from an attack of erysipelas and since then has had repeated attacks of ptosis, altogether twelve times. At some times the right eye was affected, at others the left, and at times both eyes were involved. The attacks always began with a severe headache without disturbance of consciousness. The paralysis lasted each time about four months and gradually disappeared. She had facial paralysis several times. In later years she suffered from severe headaches. On physical examination there were found: right divergent strabismus, probably congenital; complete right oculomotor paralysis, and a mild grade of right ptosis. The pupils did not react to light or in accommodation. The spinal fluid gave a positive reaction to the Nonne-Apelt and Pandy tests, and contained

forty-one cells to the cubic millimeter; the Wassermann reactions with the blood and spinal fluid were negative. The patient gradually recovered from this attack. About a year later she had another attack in which her speech became affected. There was oculomotor paralysis on the right side, with ptosis of both eyelids, more marked on the right. Both pupils were dilated and did not react to light or in convergence. There was also paralysis of the right facial and hypoglossal nerves. The patient died from an intercurrent pneumonia.

A necropsy disclosed an arteriosclerosis of the basal arteries; a small cerebral hernia; an enlarged pineal body; many small softenings; an internal hydrocephalus; the aqueduct and the third ventricle especially enlarged. The pineal body was more than twice the normal size and was surrounded by many sclerotic blood vessels. There was increased vascularity of the midbrain and, to a lesser degree, also of the rest of the brain. Enlarged blood vessels were found in the third lateral nuclei with reduction in the size and number of cells in both nuclei; at times one group and again another group of cells was affected. The median nuclei were underdeveloped and showed only a few cells but they failed to show an increase in the blood vessels. The perivascular spaces were much enlarged. The author considers it to be the result of a disturbance in the cerebrospinal fluid circulation, and the internal hydrocephalus to be due to the same cause. The primary cause of both, however, the author attributes to the enlarged epiphysis. The Darkschewitz nuclei were normal. On the other hand, the Westphal-Edinger nuclei showed swelling of the cells and were crossed by many enlarged blood vessels. There was an area of softening in the right globus pallidus and internal capsule reaching up to the corpora geniculata. A softening on the left side, embracing partly the capsula interna and partly the substantia nigra, was also found.

Summing up the observations, the author rejects the idea that the enlarged epiphysis exerted pressure on the midbrain; likewise, she denies that the enlarged blood vessels caused any disturbance in the way of pressure on the midbrain. According to her it was a congenital vascular disturbance affecting the midbrain and the epiphysis. The enlarged epiphysis is most likely to be held responsible for the disturbed cerebrospinal fluid circulation. The abnormal vascularizations she considers to be an abnormal congenital anlage of the oculomotor nuclei by crowding the space in which the ganglion cells had to develop; this affected the cells both quantitatively and qualitatively.

In interpreting the symptoms in this case, the author finds that they cannot all be explained on the basis of a single lesion. The enlarged epiphysis, even though it failed to produce pressure on the roof of the midbrain, must be looked on as having caused a circulatory disturbance in the fluid which led to a dilatation of the third ventricle and the aqueduct. There was no evidence of an inflammatory process, although this thought is suggested by the first attack of ptosis coming on at the age of 28 after an attack of erysipelas. The abnormal increase and the large blood vessels in the oculomotor nuclei can partly explain the hypoplasia of the Westphal-Edinger and the lateral nuclei.

The followers of the theory that the Westphal-Edinger nucleus controls the sympathetic innervation of the eye will perhaps find in this case support for the view since the patient failed to show pupillary reaction to light. But the author rightly points to the fact that while there was hypoplasia, a total loss of the nuclei was out of question.

According to Bernheim and Brouwer the levator palpebrae superioris is innervated by a group of cells in the frontal pole of the lateral nucleus. The ptosis in this case may, therefore, be explained on the basis of the pathologic observations in this part of the nuclei. Others are of the opinion that it is the caudal group of cells in the nuclei that control this muscle; still others believe that the nucleus exerts a synergic action and deny that separate groups of cells innervate particular muscles. The diffused and increased vascularity in this area renders the muscle localization still more difficult. It is to be

noted that, while the reduction of the large cells was found to be about the same in both lateral nuclei, there was no paralysis of the left ocular muscles, although the right was paralyzed.

The author, therefore, assumes a congenital disturbance in this case — an abiotrophy. There was formation of ganglion cells, but they had not grown with the demands of extra-uterine life; one group of cells may succumb more readily than another and the fact that ptosis first appeared at the age of 28 after an attack of erysipelas fits in well with this conception. The erysipelas acted as a provocative. The same theory can explain the strabismus in this case, as well as the cerebral hernia and the facial paralysis which the patient had had three times, since congenital disturbances are often multiple. The recurrent attacks of ptosis can also be explained on the basis of an abiotrophy. Many factors may have an unfavorable influence such as tiredness, excitement or toxic conditions.

BERNIS, Rochester, N. Y.

DISTURBANCES IN EQUILIBRIUM. PAUL SCHILDER, Jahrb. f. Psychiat. u. Neurol. **45**:160, 1927.

Regardless of whether the organism is at rest or in motion it needs relatively stabile positions and attitudes in order to accomplish purposeful activity. These relatively stabile positions and attitudes are essential for the maintenance of body equilibrium. In this sense, equilibrium may be regarded as a condition precedent to purposeful activity. It is a biologic function which, according to J. Bauer, depends on the well regulated activities of the various end-organs, of the nervous system and of the endocrine apparatus. As equilibrium may thus be regarded as a response to an external situation the question arises how this situation makes itself felt; what responses does it call forth and what kind of afferent impulses are necessary to maintain equilibrium? It would seem first that optic and tactile kinesthetic afferent impulses are absolutely essential, and to these vestibular impulses must of necessity also be added. It is also apparent that acoustic, gustatory and olfactory impulses are also of great importance in maintaining equilibrium. The afferent impulses essential for equilibrium may then be divided into two large groups: (1) psychic experiences which reach consciousness, and (2) those that play outside of consciousness. Both these groups may be subdivided. The degree of consciousness reached by the afferent impulses is by no means always the same, so that one may have impulses that reach complete consciousness and others that reach only a lower level of consciousness. On the other hand there can not be any doubt that the afferent impulses to the cord are not the same as those reaching the centers of the cortex or the mesencephalon after they have exerted their effects on the cord. Equilibrium in general is not mediated through sensory impressions nor through experiences from higher levels of consciousness but rather through experiences from primitive levels of consciousness or through unconscious regulations. Here is to be found the important difference between equilibrium and disturbances in orientation. A great part of orientation, at least as much of it as relates to the outside world, plays around full consciousness. Instinctive orientations, as far as one's body is concerned, is much more closely related to equilibrium.

Disturbances in orientation, as far as the outside world is concerned, always influence equilibrium secondarily. Disturbances in "external" orientation lead to disturbances in equilibrium when the data of the outside world contradict one's knowledge as to the position of his own body. The latter does not consist of a summation of individual impressions, sensations and representations, but on the contrary it is the individual impressions that are in relation to the entire body scheme—a point of view of great significance in the understanding of the phenomena of vertigo. It is the inability to give proper evaluation to the body scheme during activity that plays a considerable rôle in certain forms of apraxia, such as Liepmann's ideokinetic apraxia, especially in cases in which the patients experience unusual difficulties in recognizing which is right and which is left. During vertigo there occur certain sensations of movement which are referred

to the individual's own body. As Leidler expresses it, "the sensation of movement is the nucleus of the sensation of vertigo." A study of vertigo in connection with vestibular and falling reactions leads to the assumption that in addition to conscious sensations there must also exist afferent impulses that condition disturbances of equilibrium. Furthermore, analysis of equilibrium due to disturbances in perception will show that the latter cannot give rise to the former without the intervention of unconscious and purely bodily mechanisms. Therefore, to understand equilibratory disturbances due to changes in the power of perception it is essential that one be fully acquainted with those apparatuses which, independently of consciousness, serve to regulate the maintenance of body equilibrium. As a matter of fact, one may be tempted in this connection to inquire how far the sensation of vertigo is determined by reactions of movement.

After reviewing the physiology of the various anatomic apparatuses contributing to the function of equilibrium, Schilder discusses the relationship of conscious to unconscious experiences in maintaining equilibrium. Every sensory perception influences equilibrium and tonus on the one hand, and, on the other, every sensory perception is influenced by tonus. This fact well explains why resistances of various sensory impressions lead to vertigo, and also why sensations of movement are closely interwoven with every sensation of vertigo. Leidler's belief that the vestibular apparatus always participates in vertigo is confirmed and finds a wider application in Schilder's conception, which assumes that the vestibular apparatus is only one, though an important component in the total amount of afferent impulses regulating equilibrium. When the problem is viewed in this light it is clear why cerebellar lesions are frequently not associated with vertigo. In this connection it must be borne in mind that orientation is a vital function which, through tonus, has a marked effect on the vasovegetative system.

The next problem that arises is how far, if at all, the sensation of vertigo during vestibular irritation is secondary to changes in tonus. According to Schilder this is only partly true because he believes that vague sensations of vertigo do not become definite until tonus changes have occurred. Goldstein has always insisted that opposing influences of motor and sensory processes occur in a purely physiologic and not in a psychic manner. Although Schilder is in full accord with this idea, nevertheless he asks whether it is not true that the psychic also has a physiologic significance. Can any one deny that a psychic experience may reach deeply into a man's physical existence? The psyche cannot be absolutely separated from the physical. The peculiar falling and past pointing reactions in hypnotized and hysterical persons following suggestion as to the direction of the falling or past pointing bears ample proof of the inseparability of mind and body. Löwy believes that cortical components play an important rôle in pointing reactions. One must admit that cortical regulations must exist in the maintenance of equilibrium, because it is hardly conceivable that lower functions are not influenced by those at higher levels. Even Löwy and Leidler claim the unlikelihood of a difference in the vertigo of neurotic persons from that of persons with organic disease.

The vertigo in neurotic persons is an expression of failure of union—a dissociation—of psychic experiences. The expression "the mere thought makes me dizzy" expresses a physiologic truth in that it conveys the idea that a physiologic problem has become a psychologic one. Equilibrium is a complicated structure involving the entire organism; its final and most comprehensive component must be sought in the psyche. It is a physiologic problem which thus becomes converted into a moral one.

KESCHNER, New York

MUSCLE RHEUMATISM AND MUSCLE TONUS. G. GRUND, Deutsche Ztschr. f. Nervenh. **97**:10 (April) 1927.

Acute muscular rheumatism, especially lumbago, is characterized by a sudden, almost lightning-like onset with excruciating pain which reaches at once a maximum degree and develops an immediate limitation of motion in the

affected muscle group in one who has been enjoying normal health. For days the pain and limitation of motion may remain the same. Many authors lay the greatest stress on the pain and consider it a kind of myalgia. Adolph Schmidt ("Der Muskelrheumatism, Bonn, 1918) calls it a neuralgia of the sensory nerves. This conception, however, fails to explain the suddenness with which the attack sets in, especially when it is compared with ordinary attacks of neuralgia. The limitation of motion is also looked on as a defense reflex, a protection against the increased pain which any movement may develop. The fact, however, that movement develops such severe pain makes it untenable to look for the disease in the sensory nerves to the exclusion of the motor nerves and the muscles.

Erben's opinion that it is primarily a vertebral disturbance of the joint contradicts all experiences in joint diseases, which as a rule do not have such a stormy onset, aside from the fact that involvement of the joint has not been demonstrated in those cases. There are those who consider that the disease lies in the muscle itself. That it is not an inflammatory process is admitted by all. Schade thinks it is the result of a cooling or chilling of the colloids in the muscle substance; but lumbago is in parts which are least subject to chilling.

Müller considers the disease as being due to an increased muscular contraction, a hypertonus. The cause for the hypertonus, the severity of the pain, as well as the suddenness of the onset, however, remains unexplained.

Grund compares this condition to a cramp in the calf of the leg. He finds that in both the onset is sudden, and the pain acute. A voluntary movement, not necessarily of great intensity, will bring on a contraction in the musculature of the calf which is accompanied by a maximum degree of pain. Strümpell called attention to the fact that the pain in cramp is unique. Ordinarily, even the greatest degree of voluntary contraction is not capable of causing a similar degree of pain. According to Grund, the cramp depends on the innervation of the voluntary motor tracts, causing a condition of tonic contraction of the sarcoplasm, which depends on the plastic or "Sperr" tonus. This condition of "Sperr" tonus is caused by a disturbed tonus innervation, most likely the result of an upset in the autonomic innervation. In lumbago also the exciting factor is a voluntary movement; often it is only a slight bending forward. The pain appears suddenly and counter motility becomes impossible. The theory of "Sperr" tonus, as an explanation of the etiology of a muscular cramp, can be invoked also in the characteristic onset of lumbago. It will explain the suddenness of the onset, the pain, and the immobility.

There are, however, certain differences between lumbago and muscular cramp. In the first place, there is a difference in the duration. A muscular cramp lasts only a few minutes, while lumbago may persist for weeks. Again, in lumbago an attempted movement will increase the pain; in muscle cramp it tends to relieve it. It would seem as if the pain in lumbago is brought about not so much by the action of the antagonistic muscles as by the primarily contracted muscles. One gets the impression that the counter contraction develops a new cramp wave. The difference noted between the two conditions, however, is not sufficient to constitute argument against the theory that the essential pathologic process in both is the development of a "Sperr" tonus.

The cause must be sought in an upset of the automatic innervation of the muscles, having for its basis a toxic-infectious process or perhaps also a chilling of the muscles. It is more plausible to think that a release of the accumulated stimuli, damaging some of the autonomic ganglia, brings about a sudden release of energy and results in the acute symptoms, rather than to hold either the musculature or the sensory nerves responsible.

Grund, who suffers from attacks of lumbago and torticollis, reports an observation on himself. One morning, while attempting to get out of bed, he had a sudden severe pain in the small of the back with inability to move. Every few minutes the pain rose to an unbearable degree and lasted for from

one to two minutes and then gradually subsided to its former intensity. With each rise of the pain wave, an abdominal contraction occurred, causing a boardlike rigidity, but the seat of the pain remained in the small of the back and not in the abdominal muscles, which on palpation gave only a slight sensitiveness at the insertion in the symphysis. When the crest of the pain wave subsided, the abdominal muscles again became soft and free from tenderness. Palpation of the muscles of the back caused no pain; percussion over the vertebra alone was accompanied by pain. The attack lasted five days and required injections of morphine for its relief.

The author observes that contraction of the abdominal muscles must be differentiated from the attack of lumbago. Lumbago, he considers, is caused by "Sperr" tonus, and the seat of the disturbance is at the ventral area of the lumbar vertebra, at its muscular attachments, especially at the upper part of the iliopsoas. On the other hand, the contraction in the abdominal muscles is the result of a different process. It was always associated with the height of the pain wave; it was not painful; it was not influenced by the will, although reflex action is often capable partially at least, of being influenced by the will. After rejecting various theories the author comes to the conclusion that the contraction of the abdominal muscles must be explained on the basis of a contraction tonus (the ability of skeletal muscles under gradual shortening to develop a continuous contraction) which was called forth in this case secondarily by the "Sperr" tonus, since the contractions of the abdominal muscle coincided in time with the intermittent increase of the wave pain in the back.

The author takes exception to the theory that the plastic tonus and not the contraction tonus is to be ascribed to the sympathetic system. He is of the opinion that striated as well as smooth muscles are under the influence of the autonomic system. In support of his opinion he quotes the experiments of Heidenhain, Sherrington, and E. Frank. He also calls attention to the fact that muscular cramps may be influenced by the injection of atropine, which would also speak for an autonomic influence. His conclusions are, therefore, that a large part of acute muscular rheumatism is due to a disturbed tonus innervation, and most likely is the result of an upset in the autonomic nervous system.

BERNIS, Rochester, N. Y.

ALLERGY AND EPILEPSY, ANALYSIS OF ONE HUNDRED CASES. RALPH H. SPANGLER, J. Lab. & Clin. Med. **13**:41 (Oct.) 1927.

Spangler says that recent investigations point to the probability that the immediate cause of the convulsion, in so-called idiopathic epilepsy, arises from a disturbance of metabolism. Certain clinical, hereditary, immunologic and therapeutic evidence is presented which points to allergy as an etiologic factor in some cases of epilepsy. He tabulates and analyzes the clinical observations on 100 consecutive cases of epilepsy in adults (43 women and 57 men) with especial reference to allergic manifestations in the ancestors and in the patients. Children were not included in the summary since he considers it necessary, prior to the age of puberty, to exclude spasmophilia and so-called "reflex convulsions" before a definite diagnosis of epilepsy can be established. Among the summarized deductions from his tables and data are the following:

1. Gastro-intestinal hypersensitivity to cow's milk or other substitute for mother's milk appears to be a decided factor in the development of infantile convulsions in the atopic child. In this series of 100 cases of epilepsy in adults, 62 patients were breast fed, 8, or 12.9 per cent, of whom had convulsions in infancy. Of the 38 patients who were bottle fed, 24, or 63.1 per cent, developed convulsions in infancy.

2. Bottle feeding (protein sensitivity to milk or food foreign to the mother) may be a more potent factor in the development of infantile convulsions than difficult or instrumental birth. Twenty-five of the 100 patients were first-born

infants; 16 of these were breast fed of whom 5, or 31.2 per cent, had convulsions in infancy. Of the 9 first-born children who were bottle fed, 4, or 44.4 per cent, had infant spasms. Twenty of 100 were instrumentally delivered babies and 10 of these were breast fed, among whom 3, or 30 per cent, had convulsions in infancy. Of the 10 instrumentally born who were bottle fed, 8, or 80 per cent, developed infant convulsions.

3. Clinical evidences of allergy in epilepsy often show themselves early in infancy. When food is substituted for mother's milk there appear at times, especially in the child with an allergic ancestral history, periodic vomiting, cyclic diarrhea and other digestive disturbances, which may be accompanied with hives, eczema or other sensitization symptoms. Spangler points out that it is under these conditions that infantile convulsions are apt to appear. He says that so-called "reflex spasms" in infants, especially those attributed to teething, are frequently found on close analysis to be evidence of protein hypersensitivity which occurs coincidently with the change in the form of diet the child ingests when teething. In this series of 100 adult cases of epilepsy there was a history of digestive disturbances with convulsions during infancy in 32 of the patients. Twenty-four, or 75 per cent, of these were bottle fed and 8, or 25 per cent, were breast fed. Moreover, among epileptic patients he frequently sees other allergic clinical manifestations. In 54 of the 100 patients in this series the following other allergic conditions occurred; migraine, 19; hives, 18; eczema, 11; asthma, 3, and hay-fever, 3.

4. Food history, i. e., tabulation of all articles of food ingested, for the purpose of determining and eliminating the sensitizing agent, has been of more clinical value in allergic epilepsy, in Spangler's hands, than food skin tests, which have proved disappointing. Four illustrative case histories are given. Spangler says from the cases in which he has used the ketogenic or high fat content diet he believes that its value is due more to the accidental elimination of the agent to which an epileptic patient is sensitized than to the production of acidosis or ketone bodies in the system.

5. Hereditary deductions from these 100 consecutive adult patients with recurring convulsions reveals that in 88 per cent there was a history of allergy in the immediate ancestors (parents, grandparents, uncles and aunts). The total number of ancestors showing allergic manifestations, not including convulsions, was 158 as follows: 44 of the ancestors had asthma, 12 hay-fever, 17 urticaria, 8 eczema and 77 migraine. Of these, 110 were on the maternal and 48 on the paternal side. In 12 of the 100 patients who gave no ancestral allergic history, there were 5 whose ancestors had convulsions. The total number of ancestors having convulsions was 24, but in 8 of these the convulsions did not persist after childhood.

6. Certain immunologic evidence is given that points to the anaphylactic nature in some cases of epilepsy. The possibility of the transference of epileptic sensitization in the human by the transfusion of blood from an epileptic to a nonepileptic with resulting convulsions, as reported by Boston and Henry, and the convulsions occurring in animals following the injection of blood from humans suffering with epilepsy as reported by Cuneo, are cited.

7. Therapeutic evidence of allergy in epilepsy is indicated by Hajos' observations that experimental and clinical anaphylactic sensitiveness are decreased by epinephrine and by extract of the parathyroids and the posterior lobe of the pituitary, since all of these agents have been found to aid in the treatment of various cases of epilepsy. Spangler says in his experience epinephrine has been of especial value in treating series of convulsions and in status epilepticus, but that its daily use has not proved of much benefit as a preventive measure in petit mal or grand mal seizures.

8. When the allergen or causative agent cannot be determined, or, if known, cannot be removed in epileptic patients who have an hereditary or personal

history of allergy, and in whom a leukocytosis occurs after a convulsion, nonspecific desensitization by venom protein solution, when regulated by the percentage of eosinophil cells in the differential blood count, has proved of value in decreasing the patients' state of allergy.

HUNTER, Philadelphia.

TUBERCULOSIS OF THE CEREBELLUM. TYOJIRO FURUI, Arb. a. d. neurol. Inst. a. d. Wien. Univ. **29**:248 (Sept.) 1927.

Tubercle formation is more frequent in the cerebellum, especially during childhood, than in any other part of the central nervous system. According to Allan Starr, this may be due to the fact that the back of the head is frequently exposed to traumatism by falls during infancy and childhood. The trauma may predispose the cerebellum to invasion by the tubercle bacillus. It has not yet been decided whether the tuberculous process localizes in the cerebellum only after trauma or whether there is a generalized tuberculosis in the organism. In view of the part played by the meninges in the dissemination of the tuberculous process throughout the brain, it is not surprising that trauma superimposed on an already existing meningeal tuberculosis can cause a local invasion of the cerebral substance.

Most experiments in this field have been made for the purpose of investigating other phases of the question. Fieandt, for example, produced tuberculosis experimentally merely to study which cellular elements in the brain enter into the formation of the tubercles. He showed that besides the lymphoid elements the fibroblasts, especially the glial cells, took part. The nuclei underwent mitotic division, and the cells simulated polyblasts and epitheloid cells. These glia cells can act also as phagocytes to the tubercle bacilli. Kirschbaum believes that diffuse infiltration of the meninges can easily extend into the brain and emphasizes the striking lack of reactivity of the glia. This is in contradistinction to Fieandt who emphasizes the great reactivity of the glia. Bertrand and Medakowitsch hold that the glia plays an important part in cerebral tuberculosis and agree with Fieandt that glia cells can form giant cells.

Furui presents several cases investigated in an attempt to answer various questions concerned in this problem.

How a conglomerate tubercle can arise in a part of the brain which is so well protected as is the cerebellum is seen in the results of the examination of the cerebrum in three of the cases. In all of these there was a chronic fibrous meningitis which in places appeared to be healed, and which looked acute only where the pia dipped down between the convolutions. The process was most fully developed not on the cerebral side of the pia but on the outer side facing the subarachnoid space. There were no adhesions with the brain, and little change could be seen in the cortex. As in syphilis, the first part of the central nervous system to be involved by tuberculosis apparently is the subarachnoid space. This involvement is in the form of a meningitis which finally heals by fibrosis. At more or less remote spots fresh areas appear, so that in tuberculosis the situation is more or less analogous to that in syphilis, namely, a latency between the primary infection of the brain, which need not necessarily manifest itself clinically, and the later appearance of grave tuberculous changes.

The question arises of how this latency can be interrupted. This is answered by the first case. This was a child who, immediately preceding the appearance of an apparently primary tuberculosis of the brain, had suffered a blow to the occiptal region. In the three weeks between the time of injury and the death of the patient, a fresh conglomerate tubercle had developed in the cerebellum. Yet at autopsy it was found that over the cerebrum there was an old tuberculous meningeal process which had apparently run its course. Therefore the opinion of Allen Starr is confirmed with the modification that trauma can predispose a part of the brain to tuberculous involvement only when the meninges are already affected.

The second question under consideration is the condition of the tissues surrounding the tumor. It is noteworthy that the tubercle in its extension

consumes only that part of the brain which it replaces. The parts immediately adjacent to it are but slightly involved. There is primarily a disappearance of granule and Purkinje cells and a moderately extensive diminution of the fibers.

Furui also demonstrated an unusually active proliferation of the glia in the vicinity of the tumor. This was in the nature of an increase in glial nuclei which is identical with that observed in parenchymatous destruction.

If one observes the adjacent convolutions which are not directly involved by the tubercle, one sees almost no change in their shape or appearance. On close examination, however, there is seen minimal reduction in the granules, moderately severe degeneration of the Purkinje cells, definite edema, and simultaneous proliferation of the glia. In addition to structural changes, the Purkinje cells show an alteration in shape and position. They are shrunken, and the axes of the cell bodies are either horizontal or oblique instead of vertical. The latter change is due probably to pressure. On the unaffected side the changes are barely noticeable and are due less to pressure than to edema and toxic influences.

The meninges are comparatively delicate when compared to those over the cerebrum. In all cases, however, a fresh infiltrate could be demonstrated extending into the nerve parenchyma along the vessel sheaths from the meninges. Also, when a tubercle had developed, the surrounding vessels were associated with fresh granulations which extended far into the healthy tissue. Therefore, there is an extension of the process from two different directions, the peripheral meninges and the central tubercle.

On the whole one can say that the tubercle extends merely by substitution, that the defense reaction of the tissues is intense, and that in the cerebellum this reaction depends primarily on the glial cells. KAMMAN, Saint Paul.

CENTRAL CHANGES IN RECKLINGHAUSEN'S DISEASE. MAX BIELSCHOWSKY and MAXMILIAN ROSE, J. f. Psychol. u. Neurol. **35**:42, 1927.

A woman, aged 22, who had been healthy, developed within two years symptoms indicative of a spinal tumor between the fifth cervical and first dorsal segments. Owing to a striking pigmentation of the skin (café au lait spots), the tumor was thought to be a neurofibroma or a neurinoma. It was impossible to state during life whether the tumor was intramedullary or extramedullary. An injection of iodized oil 40 per cent did not shed any light on the case. The marked atrophy of the paralyzed muscles and the peculiar sensory disturbances were in favor of an intramedullary lesion. Owing to the difficulties in diagnosis, an exploratory laminectomy was performed; following this the patient died from respiratory failure. Necropsy revealed at the level indicated a solitary neurinoma within the right half of the cord. The tumor occupied almost the entire right lateral column and in some places invaded also the gray substance of the cord.

Microscopically, the tumor was a typical central neurinoma, with spindle shaped nuclei of various sizes, embedded in a finely striated stroma. There was no sharp grouping of cells. In certain areas the ground substance of the tumor showed hyaline metamorphosis with resulting syringomyelic and cystic cavitations. Immediately surrounding these hyaline areas the ground substance appeared reticular and spongy. In van Gieson preparations the ground substance took the typical reddish orange color. Throughout the sections the nuclei and fibrillae were arranged in whorls giving them a braidlike appearance. Rows of nuclei alternating with stripes of tissue free from nuclei—a condition characteristic of peripheral neurinoma—were greatly in evidence, as were the nerve fibers usually found in these growths. The elements here were almost exclusively nonmyelinated and were actually "melted" into the plasma of the tumor cells. In addition, the growth contained various kinds of ganglion cells; these were partly anterior horn cells which having first been surrounded by tumor cells began

to show various degrees of secondary degeneration, while others were atypical cells whose structure did not leave any doubt of their embryonic neuroblastic genesis.

The presence of intracellular and extracellular glia fibers attested to the gliogenous nature of the tumor. The increased number of neuroblastic elements within the region of the tumor was striking. The smaller number of grotesquely shaped cells also removed the usual doubts as to whether one was dealing with a degenerative or with a dysgenetic process. The morphology of these cells was definitely that of immature ganglion cells; their inclusion in the fibrillary substance was of no little significance in indicating that structurally the tumor approximated closely a ganglioneuroma, and that originally, in addition to the spongioblasts of the primary neural tube, neuroblastic matrix cells also participated in the formation of the tumor. The presence of these cells gives the tumor an intermediate position between malformation and new formation.

The angiomatous deposits in the tumor tissue were also unusual; they appeared partly as telengiectatic, vascular nets and partly as solid cavernomas. They are probably due to faulty vascularization and connective tissue supply of the cord, particularly of the dura and pia during organogenesis. Their histologic structure indicated that they were not be regarded as an overproliferation of mesenchymal elements.

Bielschowsky and Rose are of the opinion that the various manifestations of excessive glial formation in Recklinghausen's disease are due to faulty gliokinesis. Rests during an early termination period give rise to neuro-epithelial tumors; later the more differentiated spongioblastic material forms the substratum for neurinoma, whereas the occurrence of smaller cerebral and spinal cord foci is due to failure of development during the latest migratory period. The fact that these smallest foci may exist without the slightest deviation in morphology, even without any disturbance in the cyto-architectonic and myelo-architectonic structure of the central tissues, would seem to be in favor of this theory. This conception can be readily reconciled with Antoni's hypothesis as to the origin of peripheral neurinomas, because the ganglion layer is a mere attachment to the periventricular matrix and actually consists of the same elements as the latter. The occurrence of neuroblasts and ganglion cells in the various tumors of the nervous system is no evidence against the truth of this hypothesis. On the contrary, one must expect to find that with defective migration, differentiation and localization of spongioblastic elements a similar process may occasionally occur in neuroblasts arising from the same developmental processes. It is merely that in contrast to the spongioblasts these elements are capable only of slight proliferation; later in life they cease to be factors in neoplastic formations but form regularly inclusions within the blastomatous substance derived from spongioblasts.

KESCHNER, New York

STAMMERING. NADOLECZNY, Arch. f. Psychiat. **82**:235 (Nov.) 1927.

The author reviewed the problem of stammering before the neuropsychiatric society at Munich. It is now generally accepted that stammering belongs to the functional diseases, and the Kussmaul-Gutzmann definition could be expressed today as follows: a neurotic reaction in the field of speech coordination on a constitutional basis. The author expresses himself as being particularly opposed to those who regard stammering as some form of aphasic disturbance on the one hand, as well as those, on the other, who regard it as a pure anxiety hysteria. Of etiologic factors, the first to be considered is an inherited predisposition. There is a decided preponderance of occurrence among males. The author found 77 per cent males and 23 per cent females (in children). The frequent occurrence of stammering in members of the same family may be due partly to heredity and partly to acquired factors. Besides the constitutional predisposition, there are undoubtedly some factors in the environment that serve as etiologic agents. Stammering frequently follows

an infectious disease (measles, whooping cough, scarlet fever, diphtheria, mumps, typhoid, etc). There is evidence pointing to the fact that endocrine disturbance may bring about stammering. The most important factors, however, outside of the constitutional predisposition, are to be looked for in the environment and especially in the early years of childhood. The author does not agree with some representatives of the psychoanalytic school who place too much weight on early sexual traumas in the causation of stammering, although he believes that they are of some importance. He takes the point of view sponsored by L. Frank, Laubi and Homburger that affects of all types, such as fear experiences, unpleasant situations, sometimes anxiety states and a great many other emotional incidents added on to a constitutional predisposition, bring about stammering. A factor of importance is the tendency of children to imitate others in their environment and thus acquire the habit of stammering. The manifestation of the well known tonic and clonic inhibitions in the various muscles concerned in breathing, voice and pronunciation that are characteristic of stammering depend to a great extent on the situation in which the individual finds himself. A change in the accent or pitch generally has a good effect. For instance, it disappears in whispering or when the accent is placed on the wrong syllable. Then again, singing or rhythmic recitation seems to have a good influence. There are also a number of factors that seem to influence the condition in special cases. For instance, repeating words spoken by somebody else, or reading, may influence some stammerers. Others show a disappearance of the stammering in counting or in repeating memorized verses. Some stammerers can speak freely when alone or in a dark room. Others, on the other hand, are at their best when in front of an audience. This is especially noted in some actors who show much stammering when off the stage. Many stammerers do not show any disturbance when pronouncing words in a foreign language. One sees that the situation in which a stammerer finds himself has a great influence on his stammering. The treatment of the condition depends greatly on these factors and one should attempt to alter the situation.

Although the author agrees that an analysis of each patient for the purpose of discovering the possible causative factors is of some importance, he believes that the most important part of the treatment consists in regular speech exercises. In most cases the best method is to begin with changing the usual accent, either by singing the words or by pronouncing them rhythmically, and then to proceed to build up the desired accent. In the discussion following this review, Laudenheimer and von Hattingberg expressed themselves as disagreeing with the author on the question of regarding stammering as an anxiety neurosis. They also believed that psychoanalysis in cases of stammering is frequently beneficial and that in any case a complete analysis of the person's early experience should precede both in time and in importance the actual speech exercises.

MALAMUD, Foxborough, Mass.

A FAMILIAL DISEASE WITH SPECIAL INVOLVEMENT OF THE BASAL GANGLIA. L. KALINOWSKY, Monatschr. f. Psychiat. u. Neurol. **64**:168 (Oct.) 1927.

Among the heredodegenerative diseases, types involving the basal ganglia are not common. They include Huntington's chorea, Westphal-Strümpell's disease, and Wilson's disease. In 1922 and 1923, a new entity was added to this group by Hallervorden and Spatz. They described two cases of almost pure degeneration of the pallidum and the pars reticularis of the substantia nigra. The author has had an opportunity to make a careful study of a family showing three similar cases.

There was no history of previous nervous disease in the family. There were four children — three sons who were all affected, and one daughter who escaped. The onset occurred at the age of 9 or 10 years, with pes cavus equinus and beginning spasticity of the legs. One or two years later, there was beginning impairment of vision and progressive dementia. The fully developed picture

showed: an advanced dementia; a tendency to compulsory laughter; optic atrophy with marked but incomplete loss of vision; marked difficulty in speech — the speech being explosive and incapable of modulation; moderate rigidity of the arms without paresis; rigidity of the legs in adduction, with increased deep reflexes and inconstant Babinski sign; flexion of the knees; deformity of the feet — a combination of pes cavus, equinus and clubfoot; incontinence; normal potentia and increased libido. The whole picture is completed by extrapyramidal, hyperkinetic symptoms, differing somewhat in the three cases: (1) hyperkinesias of athetotic character in the head and hands; (2) tremor of the hands; (3) tremor of the hands and palilalia.

The oldest brother died at 26 of bronchopneumonia. The brain showed, macroscopically, general narrowing of the convolutions as far back as the parietal lobe. Frontal sections showed a shrinking of the medullary substance. The striking thing, however, was a brownish discoloration of the globus pallidus and a brownish green discoloration of the substantia nigra. Microscopically, there were diffuse changes throughout the brain and distinctly focal changes in the globus pallidus and zona reticularis of the substantia nigra. The diffuse changes included some loss of cells with glia increase throughout the entire cortex, emphasized in the precentral gyrus. The temporal and occipital lobes were relatively intact. The cerebellum showed much reduction of Purkinje cells. The spinal cord showed some diffuse pallor in the pyramidal tracts, and in the columns of Goll and Burdach. The focal changes, involving the pallidum and zona reticularis of the substantia nigra, consisted of: a loss of cells, with much damage to those remaining; a more or less diffuse glia proliferation; the presence of curious rounded structures, described in their cases by Hallervorden and Spatz and wrongly classed by them as swollen axis cylinders, and the packing of the whole stroma with pigment masses and concretions, most of which contain iron and give to these structures their characteristic macroscopic appearance. In Weigert preparations the pallidum resembles Vogt's status dysmyelinatus.

The mesodermal tissue was not involved. There was no evidence of inflammation. The process was primarily a degenerative one, and that it was still active was evidenced by the presence of fat granule cells.

That one is dealing with a heredodegenerative disease is evidenced by: the familial character; the striking uniformity of the picture in all the reported cases; the finding of degenerative changes without evidence of inflammation. Clinically, the cases are characterized by progressive dementia, optic atrophy, and on the motor side by a combination of pyramidal and extrapyramidal symptoms. Pathologically, there is elective involvement of the pallidum and the zona reticularis of the substantia nigra. The existence of similar cases, as reported by Hallervorden and Spatz, suggests some connection between the two structures involved. Mirto demonstrated a phylogenetic connection; on the anatomic side, Spatz showed a small cell column joining them; a physiologic relationship is suggested by the similarity in their iron content.

SELLING, Portland, Ore.

TUMOR SYMPTOMS IN CEREBRAL HEMORRHAGE. M. L. RICHARDSON, Jahrb. f. Psychiat. u. Neurol. **45**:298, 1927.

A woman, aged 42, who had been healthy was admitted to the hospital at the end of June, 1924, with sudden loss of consciousness, right hemiparesis and disturbance in speech (the nature of which was not stated). Examination on Aug. 6, 1924, showed: normal pupils, fixation nystagmus and slight deviation of the tongue to the right; right hemiparesis; a questionable right Babinski sign, with normal tonus and reflexes; urinary incontinence; slow and difficult speech, and a negative (?) Wassermann reaction of the blood. A few days later, persistent vomiting set in associated with psychic retardation and headaches localized to the right side. As the disease advanced there developed: exquisite tenderness of the skull; facial paresis on the left side (probably central), right

ptosis and limitation of the ocular movements especially to the left and upward; spastic paresis of both lower extremities which was more marked in the distal segments; a positive Babinski sign on the right and a suspicious one on the left; sensory disturbances on the right (nature not stated); urinary retention; normal ocular fundi. The vomiting became more severe; bronchopneumonia set in and the patient died.

Necropsy revealed a normal dura, markedly hyperemic leptomeninges and convolutions that were not flattened. The pons on the left side was enlarged, arched and of fluctuating consistence. Near the cerebral peduncle blood could be seen to trickle down through small dehiscences. A frontal section through the pons showed a large hemorrhage in its left half which had apparently destroyed the pes and tegmentum on that side. The margin of this hemorrhage showed a zone, from 1 to 4 mm. in width, adherent to the surrounding, partly destroyed, brain tissue and rich in blood pigment; this zone was yellowish brown and extended as far as the cerebellum. After removing the products of freshly coagulated blood from the large blood clot there remained a cavity the wall of which, except for a few nodules, was smooth in outline. The cerebellum seemed to be soaked through with blood. Below this the hemorrhage reached the inferior olive which had not been destroyed. The aqueduct was flattened and deformed upward and to the right by the surrounding hemorrhage. The third and lateral ventricles were moderately dilated.

Pathologically, then, the case appeared to be one of circumscribed hemorrhage in the region of the pons due to cerebral vascular disease. It may be assumed that the clinical symptoms were due to an old hemorrhage which began to organize and to heal (as evidenced by the smooth-walled pseudocystic formation), and that just prior to death a fresh hemorrhage occurred in the same area. This second hemorrhage gave rise to the terminal clinical symptoms leading to sudden cerebral edema and compression of the aqueduct with secondary hydrocephalus.

The author then describes the clinical symptoms in the light of the pathologic observations and shows why the clinicians were justified in diagnosing tumor of the brain. He presents nothing that is not well known to American neurologists, every one of whom must have made at one time or another a similar mistake in diagnosis in a case of this type. The reviewer is disappointed in the poor clinical report of the case. There is no record of lumbar puncture, urinary examination, blood chemistry, etc. Some information might also have been obtained by subjecting the patient to encephalography; this was not done.

KESCHNER, New York

TUMORS OF THE POSTERIOR FOSSA. KURT MEUSBURGER, Wien. klin. Wchnschr. **40**:1313, 1927.

Both Oppenheim and Nonne have had the experience of making the diagnosis of a tumor of the posterior fossa, only to find later or at autopsy, that a tumor did not exist. In the present article the author reports a case in which a diagnosis of multiple sclerosis was made by several men, and only at post mortem was it found that the cause of the symptoms was a tumor of the cerebellum.

Clinical History.—In 1914, the patient, a woman, aged 38, following several convulsive attacks, developed progressive right hemiplegia, which persisted for several months and then slowly improved, leaving, however, a few residuals. At that time, the condition was diagnosed as hysteria. Nine years later, for no apparent reason, the patient developed a right facial paralysis, which cleared up after several days. In 1924, there was a transient tetraplegia, which again disappeared in the course of a short time. In 1925, a temporary right facial paralysis again developed; associated with this were some headaches. It was at about this time, March 26, that the patient was first seen by Meusburger, as she again had had a right facial paralysis and complained of paresthesias over the right side of the face, the jaw and the tongue. Examination at this

time showed a moderate vertical nystagmus. Both a motor and a sensory disturbance were present in the right fifth nerve; there were a complete right facial paralysis and a marked impairment of hearing on the right side. The abdominal reflexes were absent on the left side and present only in the upper right quadrant. The right leg was spastic, with a Babinski reflex. There was an ataxic gait. The fundi were normal.

Course.—In view of the rather long history, with the apparent episodes of improvement, a diagnosis of multiple sclerosis was made. The patient was seen on several occasions during the next few months without any signs of improvement. She died suddenly on May 20, 1926. At necropsy, the examination gave negative results except for a fibrosarcoma of the right cerebellar pontile angle, arising from the substance of the cerebellum and extruding itself into the cerebellopontile angle.

Comment.—As one might expect, as a result of this experience, the author has reviewed the literature of cerebellar tumors and multiple sclerosis and has found that these conditions have frequently been confused. Lewandowsky, in his textbook, reports several cases in which a diagnosis of multiple sclerosis had been made and only at autopsy was it found that the patient had a tumor. The reverse has also been noted: cases have been diagnosed as tumors of the posterior fossa and only at autopsy was the true nature of the condition determined. The author attempts to explain the error in diagnosis as due to pressure exercised on the pons and the cerebellum by the tumor. The episodic course, he believes, resulted from the variations in the tumor mass as a result of its cystic character.

MOERSCH, Rochester, Minn.

STATISTICAL AND COMPARATIVE STUDIES ON THE CLINICAL FORMS OF GENERAL PARALYSIS AND THEIR PROGNOSTIC SIGNIFICANCE BEFORE AND AFTER THE INTRODUCTION OF MALARIA THERAPY. S. HECHT, Arch. f. Psychiat. u. Nervenk. **81**:133 (June) 1927.

This study was made on 161 cases of general paralysis (122 men and 39 women). The material is divided into groups, according to clinical types as follows: (a) Simple dementing (61.5 per cent of the men and 66.7 of the women); (b) expansive and agitated (18.8 per cent of the men and 17.9 per cent of the women); (c) depressive (5.7 per cent of the men and 10.2 per cent of the women); (d) circular (3.3 per cent of the men and 2.6 per cent of the women); (e) "galloping"—extremely rapidly progressing types which some authors regard as part of the expansive agitated type (3.3 per cent of the men and none of the women); (f) uncertain—cases that have not been observed for a sufficiently long time to determine to which group they belong (4.1 per cent of the men and 2.5 of the women), and (g) atypical—cases neurologically of general paralysis, but mentally suggestive of other psychoses, mostly paranoid and hallucinatory (3.3 per cent of the men and none of the women).

A comparison of the observations with those of other observers suggests to the author that there has been a change in the relative frequency of the different types. The classical expansive type, which in older literature was described as the most frequent form, has given place to the simple dementing type in modern literature. It still remains problematic whether this change is due to the introduction of arsphenamine treatment as suggested by E. Jones. There is not any marked difference between the proportions of the different types observed in men and in women, except that the depressive form seems to be more prevalent among women than among men.

Prognosis in the different clinical types seems to be as follows: The course is much more prolonged in the simple dementing and expansive forms than in any of the others; the "galloping" type follows the most malignant course. General "paretic attacks" occur with practically the same frequency in all types. Remissions were most frequently encountered in the expansive and simple dementing types; the apparent preponderance of expansive as compared with simple dementing types in observed remissions may be due to the fact that the

former came to the attention of the physician at an earlier stage than the latter. Remissions following malarial treatment are at least three times as frequent as spontaneous remissions; the proportions in the different groups are, however, practically the same. Early treatment is one of the most important prognostic points.

MALAMUD, Foxborough, Mass.

THE RÔLE PLAYED BY THE CUTANEOUS SENSES IN SPECIAL PERCEPTIONS. WILLIAM MALAMUD, J. Nerv. & Ment. Dis. **66**:585 (Dec.) 1927.

This article is prefaced by an interesting philosophic and psychologic discussion of perception and sensation. By perception, the author understands, "the act of becoming consciously aware of a content hitherto outside of consciousness." A definition of sensation, however, is more difficult and unsatisfactory; he concludes finally that sensations can be regarded as quantitative components of perceptions. The purpose of this interesting experimental work was to find out in what way the qualities peculiar to perception, as such, are influenced by a change in the qualities peculiar to sensations. The experiment was carried out by applying to the skin of the volar surfaces of the resting forearm (1) two hard rubber points, (2) two sharp metallic points and vice versa. An instrument insuring equal pressure in the two cases and simultaneous contact with the two points was used. The subject was to tell: (1) whether the two distances were equal in length or were longer or shorter than one another; (2) what he judged the distances between the two points to be.

The subject experienced at definite levels both quantitative and qualitative changes best described as thresholds. Four such thresholds could be established. For tactile stimuli the thresholds were as follows: (1) As the distance was gradually increased from zero, the subject was unable to discriminate between the two points, experiencing a one-point value up to 3.3 cm. The one-point experience then changed into that of a solid edgelike distance. This lasted up to 5.2 cm. (2) At 5.2 cm. the experience changed into one of a distance bounded by two points. The objects were judged as being near one another. (3) At 6.7 cm. the subject was able to appreciate more correctly the actual distance separating the objects. This was possible up to about 13.5 cm. (4) Above 13.5 cm. two points could be distinguished, but no definitely estimatable distance between the two could be experienced.

Quantitatively, these four thresholds differed with different persons but remained the same in principle. In different qualitative stimuli — for example, pain — the first three thresholds were at a uniformly higher level than for tactile stimuli, whereas in the fourth threshold the conditions were reversed. By substituting heat, cold and electrical stimuli for those of pain, the results were practically the same. A series of experiments with stimuli of different geometric configurations — as circles, squares, etc., but otherwise of a tactile nature — did not give positive results. The author proposes to investigate more elaborate spacial perceptions, as he thinks that new avenues of approach to neuropathologic and psychopathologic material may thus become available.

FERGUSON, Philadelphia.

ENCEPHALOGRAPHIC FINDINGS IN SKULL TRAUMAS. M. FISCHER, Arch. f. Psychiat. **82**:403 (Dec.) 1927.

The author reports the results of encephalographic examinations in fifteen patients with histories of injury of the skull. According to the observations the cases are divided into five groups: 1. Clinically established organic sequelae that showed definite deviations from the "normal" encephalographic picture. In the seven cases comprising this group, therefore, the encephalographic examination merely served as a substantiation of the neurologic examination. 2. Cases that were clinically diagnosed as purely functional and in which the encephalographic examination did not show any change. 3. A case in which the encephalographic picture showed marked deviations from

the "normal," and clinically was both objectively and subjectively normal. 4. Cases in which the objective neurologic examination did not show organic sequelae, but subjectively the patients had numerous complaints and the encephalographic picture was negative. 5. Cases in which there was a history of a definite injury to the brain with definite neurologic observations pointing toward organic sequelae, but in which the encephalographic picture did not show any deviations from the "normal."

The author came to the conclusions that: 1. The encephalographic observations should not be overestimated because some deviations are found in patients without a history of injury and in whom there are not any complaints. 2. In contradistinction to some observers who claim that symptoms that are generally regarded as purely functional (pseudodementia, reactive depression, total analgesia, tremor and vertigo) show definite encephalographic changes, the author did not find consistent deviations in such cases. 3. The neurologic examination should not be neglected or underestimated because of encephalographic observations. 4. It is possible that adaptation and compensatory mechanisms are responsible for the absence of symptoms in cases in which the encephalographic picture points to a definite organic lesion.

In all the examinations the air was introduced by the suboccipital route. To avoid unpleasant sequelae to the operation, the patient was given 0.5 cc. of caffeine soda benzoate and from 0.5 to 1 mg. of atropine or scopolamine and 0.02 cc. of pantopon subcutaneously one-half hour before the injection of air. The fear expressed by some authors that the procedure may cause psychogenic aggravation of the clinical picture in psychopathic patients is, according to the author, unfounded. Some authors have even found an amelioration of the psychogenic picture following the injection of air.

MALAMUD, Foxborough, Mass.

AMNESIA, EPILEPTIFORM CONVULSIVE SEIZURES AND HEMIPARESIS AS MANIFESTATIONS OF INSULIN SHOCK. W. L. MILLER, Am. J. M. Sc. **174**:453 (Oct.) 1927.

The case is reported of a patient who had been diabetic for two and a half years and who presented at times not only the ordinary symptoms of insulin shock, but also the more unusual symptoms of dysarthria and amnesia and during one severe reaction presented convulsions and hemiparesis in addition to these symptoms. This attack was first noted at 9:30 p. m. when the patient was found sitting in a chair, with the eyes closed and the muscles flaccid; he was apparently unconscious. He remained in a state resembling profound alcoholic intoxication until midnight when he had intermittent attacks of generalized tonic and clonic convulsions which lasted about an hour. At 2 a. m., marked weakness of the right arm and leg with flattening of the right side of the face were noted; the abdominal reflexes were abolished; the pupils were dilated and did not react to light; he showed a tendency to fall to the right. Blood taken early during the attack showed a sugar content of only 0.03 per cent. The following morning, the patient was fully rational but did not remember any incidents that had occurred during the attack, nor for a period of time preceding it.

The various theories of insulin shock reactions are reviewed and the author is of the opinion that the reaction is not due altogether to hypoglycemia. Insulin shock seems particularly prone to occur in persons with unstable vegetative nervous systems, and many of the phenomena of shock—sweating, tremor, pallor, dilatation of pupils, changes in pulse rate and abdominal cramps—can be explained as disturbances in the autonomic domain. The physical chemistry of the blood and tissues and disturbance of the acid-base equilibrium is also considered a common factor in the production of the reactions. This view is favored by the author who points out the analogy to the reaction of tetany

in which there appear to be: (1) instability of the acid-base equilibrium, and (2) disturbance of the relative distribution of the electrolytes in the blood and in the tissue fluids.

KUBITSCHEK, Philadelphia.

ABSCESS OF THE BRAIN IN INFANTS UNDER TWELVE MONTHS OF AGE. HEYWORTH N. SANFORD, Am. J. Dis. Child. **35**:256 (Feb.) 1928.

The author reports two cases of abscess of the brain in young infants and analyzes seventeen cases collected from the literature.

CASE 1.—An infant, aged 3 months, was sent to the hospital with a condition diagnosed as hydrocephalus. Birth was at full term and delivery was normal. The mother stated that the child's head was large at birth, but that three weeks prior to the child's admission to the hospital it had grown rapidly. The child was well nourished. The temperature was 99 F. and the patient was not acutely ill. There was a marked hydrocephalus. The spinal fluid was a little cloudy; the Wassermann reaction was negative. During the week following entrance into the hospital convulsions occurred. The second spinal puncture yielded fluid containing 244 cells. Two days after admission to the hospital an operation was performed. The corpus callosum was pierced and about 3 ounces (89 cc.) of pus was obtained. Following the operation, projectile vomiting occurred with two or three convulsions daily. Forty-two days after entrance into the hospital the patient died. Autopsy showed multiple abscesses of the brain with a large abscess formation over the entire left hemisphere. No evidence of an infective process elsewhere in the body was determined.

CASE 2.—In an infant, aged 47 days, born at full term by normal delivery, the mother noticed several convulsions accompanied by twitching of the arms and rolling of the eyes. Physical examination showed that the child was well nourished. The temperature was found to be normal. There were jerking movements of the left arm, twitching of the left lip and a left ankle clonus. The results of a physical examination were otherwise negative. The Wassermann reaction, Pirquet reaction and urinalysis were all negative. After a short stay in the hospital, the patient appeared better. Convulsions, however, soon began again. Spinal puncture revealed the presence of *Staphylococcus aureus*. The convulsions increased in intensity and number, and the patient died about thirty days after the onset. Autopsy showed an abscess of the right cerebral hemisphere which occupied about four fifths of this area of the brain. Evidence of an infective process was not found anywhere in the body.

VONDERAHE, Cincinnati.

OCULAR DYSMETRIA. C. ORZECHOWSKI, J. f. Psychol. u. Neurol. **35**:1, 1927.

Ocular dysmetria is a form of hyperkinesia in which the eyeballs move in chaotic fashion, so rapidly and with such varying intensity that it is almost impossible to analyze the movements. Orzechowski designates the movements as an "ataxic dysmetria of the eyes." The phenomenon is elicited as follows: On asking the patient to fix his eyes on an object placed to one side of the visual field he is unable to do so because the eyes go beyond the object, anywhere from 1 to 2 mm.; during this process of "overreaching" the eyeballs can be seen to execute several pendulous oscillations, after which they fix normally. The inability to fix normally as soon as the eye looks at the object interferes greatly with vision. The patient has great difficulty in distinguishing objects clearly, especially when they are being moved. In order to see objects clearly it is necessary that they remain fixed. Reading is difficult because the patient must constantly move the head and eyes from the right to the left.

This form of ocular dysmetric ataxia is not to be confused with nystagmus. Contrary to what is ordinarily observed in nystagmus this form of ataxia pre-

sents only one or two movements, frequently unequal, and it disappears when the eyeballs are sustained in extreme positions. Marked ocular dysmetria is rare, whereas a mild form is often observed in cases presenting a cerebellar symptomatology. Ataxic dysmetria also differs from opsoclonia (myoclonic ataxia) which has only recently been described by some Polish observers in cases following nonepidemic encephalitis. In myoclonic ataxia the ataxic movements are more brusk in character and are clonic in nature; they appear at the onset of movement, persist throughout the entire duration of movement and cease when the eyeballs become fixed. One also encounters mixed cases that are extremely difficult to analyze because the ataxic dysmetria is complicated by myoclonic movements.

Orzechowski believes, on theoretical grounds (he offers no pathologic proof), that dysmetric ataxia of the eyeballs is due to involvement of the superior cerebellar peduncles, whereas in opsoclonia the lesion is probably in the dentate nuclei of the cerebellum.

The original article is in French. KESCHNER, New York.

THE EFFECT OF THYROID THERAPY ON CHILDREN. ANNE TOPPER and PHILIP COHEN, Am. J. Dis. Child. **35**:205 (Feb.) 1928.

The authors studied the effect of thyroid therapy on normal children and on a small series of children showing hypothyroidism. The authors began to use thyroid therapy in cases of nephrosis in children. Using basal metabolism determinations as a criterion of the effect of thyroid administration they found that no change occurred unless an infection supervened. They then extended their studies to other children. Each child was kept in bed during the study and the diet remained unchanged. The basal metabolism was then determined on several occasions until a normal reading was obtained for each patient. Thyroid extract, one-quarter grain (0.16 Gm.) three times daily, was given. The dosage was then gradually increased to 1, 2, 3 and 5 grains (0.06, 0.13, 0.19 and 0.32 Gm.) three times daily, and the basal metabolic rate was determined after each change in dosage. In four children with subnormal thyroid activity, thyroid in small doses promptly brought the basal metabolic rate to a normal level. In nine children with a normal basal metabolic rate as much as 15 grains (0.07 Gm.) of thyroid extract daily did not increase the basal metabolism. Four of these children, however, in whom careful measurements were taken, showed a remarkable increase in growth in this period. Since thyroid extract in relatively large amounts does not seem to have any effect on the basal metabolism of normal children, the authors question whether this test is a reliable criterion of the effect of thyroid therapy in childhood. The authors suggest that the growth of the child and other physiologic and metabolic responses are of greater value. On the basis of their work the authors put forth the following theory to explain the difference between thyroid action in the normal adult and in the normal child: thyroid is a metabolic catalyst and increases the phase of metabolism which is dominant in the individual—anabolic, or growth processes, in the child, and catabolic, or oxidative processes, in the adult. This theory is in line with Kendall's opinion that thyroxin speeds metabolism in the direction in which it is going.

VONDERAHE, Cincinnati.

THE CONTROL OF THE MELANOPHORES IN THE FROG. BENJAMIN KROP, J. Exper. Zool. **49**:289 (Nov. 5) 1927.

An attempt was made to ascertain what internal coordinating systems are functional in the case of changes of color in the frog. Noxious stimuli, such as scratching or pricking of the skin, cause contraction of melanophores at the place of irritation. This effect is brought about by direct action of the stimuli on the pigment cells. In conditions of anemia, brought about by

occlusion of the blood, the melanophores assume a reticulate condition. Pigment motor fibers enter the skin of the leg in the region of the knee and run to the foot. On severing these the melanophores of the web usually expand. Injection of epinephrine, either before or after the sciatic artery is occluded and the sciatic nerve cut, causes darkening of the operated leg within five minutes. The reason for this was not determined. Ether and chloroform cause expansion of melanophores of frogs. Chloretone causes expansion of melanophores of tadpoles.

The eyes of frogs and tadpoles are the chief receptors for stimuli which cause adaptive changes of melanophores. The peripheral spinal nerves and the sympathetic roots forming the sciatic plexus contain pigment motor fibers to the leg. Stimulation of these nerves causes disruption of the melanophores of the web. Melanophores of the tail of the tadpole of *Rana clamitans* are supplied by fibers from the peripheral spinal nerves. The melanophores of the iris of the frog's eye are under direct nervous control. The skin of the frog is sensitive to light and melanophores may be affected through the skin. The eyes are of relatively greater importance than the skin in receiving and mediating light stimuli. The reactions of the chromatophore system of the tadpole resemble, in general, that of the fish more closely than it does that of the adult amphibian.

The blood is the coordinating mechanism for facilitating rapid contraction of melanophores in response to noxious stimuli and in conditions of excitement. Such contraction is brought about through reflex stimulation of the suprarenal glands.

WYMAN, Boston.

CAPILLARY FORMS IN THOSE MENTALLY DEFICIENT AND THEIR RELATIONS TO PSYCHIC DEVELOPMENT. H. K. KAHLE, Arch. f. Psychiat. **81**:629 (Sept.) 1927.

The author reports an investigation of capillary forms in 507 inmates of an institution for feebleminded children as compared with a similar investigation of ninety-two normal school children. The investigation was conducted along the lines suggested by W. Jaensch. It was found that only five of the normal children showed abnormal capillary forms; all the others belonged to the normal groups. Three of the five were reputed to be backward children. The feebleminded children could be divided into three groups according to the capillary types: (1) The first group consisted of children showing definite archicapillaries as described by Jaensch, comprising 17 per cent of all the feebleminded children. (2) This group included 53.4 per cent of the feebleminded children. They showed deviations from the normal which do not correspond to any of the types described by Jaensch—broadening of the loops, widening of the capillaries themselves, and a pathologic uniformity and simplicity of the shape of the capillaries. (3) This group, consisting of 29.6 per cent, represented the children with capillaries as near the normal type as were found in the institution. Even in them, however, there were slight deviations from the normal. A comparison of all of these with the pictures of the normal children, shows that the capillaries of practically all the feebleminded were different from those of the normal children.

The author comes to the conclusion that examination of the structure and shape of the capillaries is of value in diagnosing feeblemindedness. Furthermore, the type of pathologic process found, whether nearer to the primitive archicapillaries or to the normal type, is indicative of the degree of mental deficiency. It is an important guide in an attempt to train and teach the feebleminded child, and in cases in which the mental deficiency is associated with possible endocrine disturbances close observation of the development of the capillaries under treatment is a useful guide and indicator of progress.

MALAMUD, Foxborough, Mass.

THE GENESIS OF EPILEPTIC ALBUMOSEMIA AND THE INTESTINAL DIGESTION OF PROTEINS, CARBOHYDRATES AND FATS. GEROLAMO CUNEO, Note e riv. di psichiat. **14**:381 (Sept.-Dec.) 1926.

The author, by biochemical experiments, proves that in addition to epilepsy of cerebropathic origin, there exists also an autotoxic epilepsy, which is a morbid entity clearly distinct from the cerebropathic form. It is functional in nature, and has an extracerebral origin and well defined, specific autotoxic characteristics all its own. It is caused by functional alteration of the organic chemism which produces determined endogenous poisons. These exercise a convulsive action on the brain but are not formed in it.

In all these cases he found proteosemia and a great quantity of organic acids not burned or transformed into sodium carbonate. These two observations are always associated with each other. The proteosemia was not produced by the alimentary proteins of intestinal digestion, but its genesis was connected with that of acid intoxication. In fact, a diet rich in protein improved the marked condition, and a diet free of protein but rich in starchy substances made the condition worse, while a diet free of protein as well as starch, and consisting exclusively of amino-acids, fats and sugar, given for nine days, produced a complete suppression of the attacks.

By investigating the intestinal digestion with the addition of ammonium carbonate, he observed that the diets rich in protein, sugar and fats, presented an analogous behavior; that is, they did not produce an acid intoxication and were accompanied by an amelioration of the morbid state. A diet rich in starch produced a marked acid intoxication and an increase of convulsive seizures. When pancreatin was added to the diet, the morbid condition, as well as the chemical observations, were so aggravated as to oblige him to suspend the experiments.

Since the complex chemistry of digestion and nutrition is so profoundly altered in these cases, he is inclined to attribute the autotoxic origin of epilepsy to metabolic disturbances.

VINCIGUERRA, Elizabeth, N. J.

THE PATHOGENESIS OF MONGOLIAN IDIOCY IN THE LIGHT OF MONGOLISM OCCURRING IN TWINS. KNUD H. KRABBE, Acta Psychiat. et Neurol. **1**:337, 1926.

Mongolian idiocy is usually considered to be the result of some degenerative process affecting the child during fetal life. It has not been reported often as occurring in more than one member of a family, but there are a number of reports of cases in which one twin was a mongol and the other normal. Only three instances have been reported in which both twins suffered from mongolian idiocy. The author reports five examples of mongolism in twin children. In four instances, one twin was a mongol, the other, normal. In the fifth case both twins were mongols. The sex distribution was as follows: (1) female mongol, male normal; (2) female mongol, female normal; (3) male mongol, male normal; (4) male mongol, female normal; (5) female mongol, female mongol.

The fact that one child of twins may suffer from mongolian idiocy and the other be normal seems to indicate that the mongolism is not due to action of any toxic influence on the child in utero. In two of the five instances cited by the author the twins must have been biovular because the sexes are different; it seems probable that in the other two instances in which one twin was normal, the children were the result of a biovular pregnancy although the sexes were the same. It is probable, therefore, that, in the case of the mongolian twins reported, the children resulted from a monovular pregnancy because the sex is the same.

It has been found almost constantly that children suffering from mongolian idiocy are born of mothers who are advanced in years. The author concludes that some degenerative process, possibly the result of age, affects an ovum that develops into a mongolian child, and that the condition is determined by the state of the ovum before impregnation.

PEARSON, Philadelphia.

CHRONIC PSEUDO-APPENDICITIS DUE TO INTERCOSTAL NEURALGIA. J. B. CARNETT, Am. J. M. Sc. **174**:597 (Nov.) 1927.

The literature dealing with the operative results in chronic appendicitis is reviewed, and the percentage in which relief was not obtained is stressed. Carnett states that he has about arrived at the heretical conclusion that there is no such clinical entity as chronic appendicitis. He is of the opinion that the source of the pain and tenderness in many of these patients is in the abdominal wall rather than within the abdomen and offers a simple bedside test to differentiate between intra-abdominal and extra-abdominal tenderness. This consists in palpation while the abdominal wall is held tense as well as with the muscles relaxed. If the tenderness is elicited while the abdominal wall is tense, the pain is of parietal origin, whereas pain of intra-abdominal origin should be elicited only when the abdominal muscles are relaxed. Pain and tenderness of the anterior abdominal wall is considered commonly to be due to intercostal neuralgia, especially when chronic in nature. The author uses intercostal neuralgia in a broad sense, including all lesions of the cord, meninges, vertebrae and nerves which can give rise to pain in the intercostal and first lumbar nerves; he considers it a common condition. This condition produces a pseudo-appendicitis often mistaken for chronic appendicitis, but careful examination by pressure and pinching of the skin will reveal points of tenderness along the upward course of the nerves and permit a differential diagnosis.

KUBITSCHEK, Philadelphia.

QUANTITATIVE ASPECTS OF TONIC IMMOBILITY IN VERTEBRATES. HUDSON HOAGLAND, Proc. Nat. Acad. Sc. **13**:838, 1927.

Tonic immobility or "death feigning" is shown by vertebrates as well as by a great variety of animals. This phenomenon is considered as an escape reaction and from certain standpoints is comparable to the state of decerebrate rigidity. By an appropriate technic the author studied the mechanism of tonic immobility of lizards (*Anolis carolinensis* and *Phrynosoma strumosa*). "An automatic mechanical method was developed for controlling extraneous variables connected with arousal or excitation, of regulating the temperature, and of recording periods of immobility." On the basis of results obtained the author assumes the existence of two autacoids, α and β, which are conceived as "decomposing to active chalones" (inhibitory hormones) *A* and *B*. The latter substances appear to favor the passage of impulses from the tonic centers, at the same time inhibiting the higher centers. This mechanism is consistent with the little that is known of the physiology of tonus in skeletal muscles and of the nature of inhibition. The author summarizes his results as follows: "Analysis of tonic immobility in the lizard and *Anolis carolinensis* leads to the assumption of the existence of two independent inhibitory hormones. These hormones are assumed to produce tonic immobility, when present above threshold concentrations, by inhibiting impulses from higher nervous centers and by allowing impulses to pass from the tonic centers to the muscles.

YAKOVLEV, Palmer, Mass.

LIVER DYSFUNCTION IN MIGRAINE. J. S. DIAMOND, Am. J. M. Sc. **174**:695 (Nov.) 1927.

The various theories of migraine are briefly reviewed and the results of liver function tests in a series of thirty-five patients is reported. This group was composed of twenty-one females and fourteen males. Nearly all gave a family history of the disease and the transmission was usually through the mother. Twelve cases were of the pure cephalic type, associated bilious attacks occurred in fifteen, and in eight, gastric and abdominal symptoms, the abdominal or visceral migraine, predominated. No case was included which showed other

pathologic processes or conditions that might produce symptomatic migraine. The test included the van den Bergh estimation of bilirubin in the serum and the urobilinogen test in the urine.

Evidence of disturbed liver function was found in thirty-two of the thirty-five cases, the bilirubin retention being from 1 to 5 units (0.4 to 0.8 unit being normal). The reactions were lowest in the group with simple hemicrania, higher in those with bilious attacks and highest in the group classed as abdominal migraine. Constipation of the spastic type and low blood pressure were also constant observations. The physiologic mechanism of intestinal toxemia and liver dysfunction and the anaphylactic nature of the attacks are discussed and the beneficial results of treatment directed against this disorder is cited. Treatment consisted largely in abstinence from animal proteins, in changing the intestinal flora and in relief from the spastic constipation.

KUBITSCHEK, Philadelphia.

A NEUROMOTOR APPARATUS IN THE CILIATE DILEPTUS GIGAS. J. P. VISSCHER, J. Morphol. **44**:373 (Dec. 5) 1927.

Dileptus gigas is one of the largest of the more common ciliated protozoa. Material was fixed in Schaudinn's fixing fluid and stained with either acid borax carmin, or with iron hematoxylin. A system of fibers was found which is probably a neuromotor apparatus. A distinct elongated basal rod, the motorium, is located near the base of the gullet. A set of heavy fibers, arising from the motorium, radiate out somewhat symmetrically and surround the funnel-shaped gullet. A pair of heavier fibers pass directly from the motorium to the band of trichocysts, one on each side, extending to the tip of the proboscis. A set of delicate branching fibers arises near the posterior region of the motorium and becomes distributed over the surface of the organism. This system of fibers probably has a neuromotor function, because the general structure and appearance of the fibers suggest a neuromotor function and there is little or no evidence indicating any other function. The fibers in this system are also connected to the most highly specialized structures in *Dileptus* and are similar to structures found in other forms for which a neuromotor function has been demonstrated. Far from being a simple, single cell, lowly protozoon, *Dileptus* should be considered as a specialized type of cell, in which different parts of the protoplasm are differentiated into systems for the performance of different functions.

WYMAN, Boston.

SEGMENTAL TROPHIC OEDEMA OF CEREBRAL ORIGIN. ALFRED GORDON, J. Nerv. & Ment. Dis. **66**:381 (Oct.) 1927.

A case is reported of trophic edema of the left hand—due to a vascular lesion situated probably in the superior parietal lobule of the cortex — in which astereognosis of the left hand was the first clinical symptom. There were no neuritic or spinal manifestations. The onset was sudden, with slight weakness of the left hand; increased tendon reflexes developed on the left side and loss of deep sensibility over the left hand, forearm and arm. Three months after the onset of these symptoms, an increasing swelling of the dorsum and palm of the left hand developed, producing a flexed position of the fingers. The author considers it a case of "trophoedème" of Meige — a chronic edema because of its slow development and the quantity of connective tissue hyperplasia. It resembled a fruit engorged with juice (main succulente) and showed absence of prolonged pitting. He distinguishes it from the edema of cerebral hemiplegia by the fact that it does not occupy the entire paralyzed limb, the absence of pitting, and the cyanotic color and consistency of the part. Injections of pilocarpine and epinephrine on both arms showed no difference; hence it is concluded that the peripheral cause for the edema has been eliminated. The case described could be due only to involvement of the peripheral, bulbo-

medullary or cerebral portions of the nervous system, and the author suggests that trophic centers may be intimately associated with, and perhaps dependent on, sensory centers.

H. H. Hart, Greenwich, Conn.

The Motor Nuclei in the Cervical Cord of the Albino Rat at Birth. A. W. Angulo Y Gonzales, J. Comp. Neurol. **43**:115 (April) 1927.

The author has made a careful study of the cells of serial sections through the cervical region of seven spinal cords taken from rats immediately after birth. The study of sections under the microscope was augmented by a study of photomicrographs of every fifth section taken from the most perfect series. Some of these photomicrographs are shown.

The various columns of cells are designated by Arabic numerals, and a diagrammatic representation is shown of the distribution of the cells of the several columns. A clear schematic representation of a total of thirteen columns of cells shows not only their mass but their relative lengths, positions and magnitudes in all levels of the cervical cord. Some columns are found throughout the entire length of the cervical cord, while others are found only in the cervical enlargement. In some columns a degree of fusion exists. No segmental arrangement of the motor cells was found corresponding to the segmental arrangement of the spinal motor nerves. The studies have led the author to remark that the progressive embryologic development of these columns must be carefully considered in a study of their functional significance, for he believes that progressive morphologic differentiation of the musculature is associated with, or perhaps dependent on, anatomic differentiation of these cell columns.

Stone, New Haven.

Studies on Grafts of Embryonic Tissues of the Rat on the Chorio-allantoic Membrane of the Chick. I. Differentiation of Ectodermal Derivatives. Yoshi Kuni Hiraiwa, J. Exper. Zool. **49**:441 (Nov. 5) 1927.

Eleven-day rat embryos were divided into anterior, middle and posterior thirds, and each third was grafted on the chorio-allantoic membrane of eight or nine-day chick embryos. The grafts recovered after eight or nine days showed some degree of differentiation. Epidermis, hair follicles, cartilage and bone differentiated to a degree comparable with that of normal rat embryos of corresponding age; nasal sacs and mesonephros showed considerable differentiation; nervous and entodermal structures exhibited practically no capacity for independent differentiation.

The general lack of capacity for independent differentiation may be inherent in the mammalian embryo or may be due to the fact that the embryos were cultivated on a foreign host, which had also a higher temperature than the rat. Different tissues and organs exhibited different capacities for independent differentiation, to be attributed possibly to differences in their specific properties at the time of grafting. The amount and kind of tissues or organs differentiated and the degree of differentiation bore a general relation to the level of the embryo from which the graft was taken.

Wyman, Boston.

Four Years Experience with Tryparsamide in General Paralysis. J. A. Sicard and J. Haguenau, Ann. de méd. **22**:377 (Nov.) 1927.

The neurotropic qualities of tryparsamide induced the author to try this American preparation in the treatment of general paralysis, though its action against the treponema was not as pronounced as other arsenic preparations. Approximately 70 Gm. was injected during one year in weekly intravenous injections of 3 Gm. with an interval of six weeks after each period of nine weeks' treatment. Twenty patients were treated and observed, during a period of four years, with the following results: two deaths, two treatments interrupted. Two patients who had already improved before beginning the treatment

are now clinically and serologically cured. One case in which the diagnosis was doubtful and one case in which it was positive, resulted in cure. Seven patients improved clinically but the serologic tests remained positive. In four cases, the condition progressed slowly without clinical or serologic improvement. In other words, in one positive case (5 per cent), the patient was cured; seven patients (35 per cent) improved only clinically and could return to their former professional activities, while the serologic condition remained positive; the dysarthria persisted though it was diminished and the ocular syndromes were not changed at all. Compared with other methods the results obtained impressed the authors as highly favorable and justified a continuation of the experiments.

WEIL, New York.

EPIDEMIC ENCEPHALITIS: AN ANALYSIS OF FIFTY CASES. T. P. SPRUNT, Am. J. M. Sc. **174**:660 (Nov.) 1927.

An analysis is made of fifty cases with the chronic manifestations of epidemic encephalitis, with special search for predisposing factors in this disease, but no evidence was found of a constitutional predisposition or of a liability to nervous or infectious disease. The onset of the illness showed all variations from the most insidious to the most fulminant types, but it is noteworthy that 77 per cent of those patients with severe onset later developed a parkinsonian syndrome as compared to 28 per cent of those with mild onset. The symptoms of onset were in the following order of frequency: fever, diplopia, somnolence, insomnia, delirium and ptosis. Motor disturbance was far more frequent than sensory and only a few definite disturbances of the autonomic nervous system were found. Of thirty-five patients who were followed for periods of from seven to ten years, one died a suicide, twelve are able to work and twenty-two are chronic invalids. Treatment included rest, reduction of both physical and mental effort, psychotherapy, hydrotherapy and muscle training. Hyoscine hydrobromide was by far the most effective of all drugs used in medication.

KUBITSCHEK, Philadelphia.

ENCEPHALITIS PERIAXIALIS DIFFUSA. HEINRICH KOGERER, Jahrb. f. Psychiat. u. Neurol. **45**:109, 1927.

A housemaid, aged 23, was admitted to Wagner-Jauregg's Clinic twenty-one months after the onset of an illness with complete amaurosis, bilateral hemiplegia, more marked on the right side, progressive loss of tonus and "moria." The condition was diagnosed as a cerebral neoplasm involving both pyramidal tracts and the forebrain, although all the symptoms could not be accounted for by one lesion. The symptoms progressed and the patient died six weeks after admission. Necropsy revealed multiple lesions in the white substance of the brain. Histologic examination fulfilled all the requirements of encephalitis periaxialis diffusa (Schilder's disease). Against the diagnosis of a degenerative form of diffuse sclerosis as well as against gliomatous and other inflammatory processes (acute multiple sclerosis) were the well preserved configuration of the brain (macroscopically) the extensiveness of the process and the sharp limitation of the lesion to the white substance of the brain, with complete preservation of the gray matter and of the arciform fibers. Microscopically, there was destruction of the myelin sheaths with well preserved axis cylinders, increase of the glia fibers, formation of spindle shaped and granular cells as well as marked involvement of the vascular apparatus with characteristic perivascular infiltration.

KESCHNER, New York

SIMULATION OF DECEREBRATE RIGIDITY IN TWO PARETICS. CHARLES E. KIELY, Am. J. Syph. **11**:532 (Oct.) 1927.

Two cases of general paralysis with signs approaching decerebrate rigidity are presented. The first patient was a man, aged 36, who presented the usual neurologic and laboratory symptoms of paralysis and lay unconscious in com-

plete rigidity; the greatest force did not alter his position. The axillary temperature was 105 F.; the pulse rate was 160. The following day some relaxation had occurred; after a few days the patient was back to his ordinary interconvulsive state. Such attacks were recorded as having occurred a number of times previously. The second patient was a girl, aged 15, likewise with typical general paralytic symptoms, who had generalized rigidity and conjugate deviation of the head and eyes to the right. The arms were not pronated. Flaccidity of the trunk and arms occurred under anesthesia and lumbar puncture, but knee flexion was not possible. The temperature rose to 107 F. and death soon followed. Postmortem examinations were not obtained in either case, but the assumption is that both lesions were thromboses in the midbrain.

ANDERSON, Philadelphia.

PACHYMENINGITIS HEMORRHAGICA, AN EXPERIMENTAL STUDY. I. J. and I. K. PUTNAM, J. Nerv. & Ment. Dis. **65**:260 (March) 1927.

The authors studied pachymeningitis hemorrhagica in three ways: by the subdural injection of 2 cc. of blood; by examining patients dying after intracranial operations, and by feeding alcohol over long periods to animals. Five of eighteen cases did not show any membrane after blood injection; in all cases there was much less clot than might have been expected. One almost constant feature was that the mesothelial lined spaces were usually empty or contained a little blood or debris. These lesions resembled the progressive lesions of pachymeningitis in appearance but not in behavior. Injection of irritating substances subdurally produced a fibrous scar with adhesions only. Specimens were taken from the preserved duras of eighteen patients dying at various intervals after intracranial operations. Rarely, a definitely pigmented scar was found but never a lesion of significant size. Experimental chronic alcoholism never produced a subdural membrane in dogs. Experimental studies thus throw little light on the progressive subdural hemorrhage shown by clinical and pathologic observation.

HART, Greenwich, Conn.

SYPHILITIC MENINGITIS IN INFANTS AND YOUNG CHILDREN. J. W. AMESSE and W. W. BARBER, Am. J. Syph. **11**:544 (Oct.) 1927.

Four cases of syphilitic meningitis in children are reported because of the relative rarity of these cases in the literature. The ages of the patients were 3 months, 10 months, 3 years, and 6 years, respectively. The tentative diagnosis in three of the four cases was tuberculous meningitis, an error which probably occurs frequently. Three patients had positive Wassermann reactions of the spinal fluid; one had a negative Wassermann reaction, although there was a suggestive colloidal gold curve. In this connection the authors state that both complement-fixation tests of the blood and spinal fluid may be negative. Two of the patients died; one was discharged as unimproved; and the fourth was discharged after four days with considerable improvement (this being the case with the negative test of the spinal fluid). In addition to the case reports, a brief summary is given of the symptomatology and pathology of this disease.

ANDERSON, Philadelphia.

AMYOTROPHIC SYPHILITIC MENINGOMYELITIS: REPORT OF TWO CASES WITH ARGYLL-ROBERTSON PUPILS. THEODORE T. STONE, J. Nerv. & Ment. Dis. **66**:595 (Dec.) 1927.

The author reviews briefly the literature and the symptoms of this disease and reports two cases in which the patient has been benefited by antisyphilitic treatment. His summary is: (1) syphilis is the most common cause of chronic spinal progressive muscular atrophy of the atonic type; (2) the reflexes are diminished or abolished in the extremities where wasting of muscles occurs and where there is no sensory loss in most cases; (3) bulbar symptoms do

not occur; (4) there may be interference with sphincter control; (5) early and vigorous antisyphilitic treatment may bring about an arrest of the wasting and weakness; (6) the presence of Argyll Robertson pupils does not necessarily mean a poor prognosis.

FERGUSON, Philadelphia.

THE PREVENTION OF TETANY BY THE ORAL ADMINISTRATION OF AMMONIUM CHLORIDE. W. F. WENNER, Am. J. Physiol. **81**:612 (Aug.) 1927.

In this investigation a series of six thyroparathyroidectomized dogs was utilized. After the operation these animals were given from 100 to 200 cc. of 5 per cent ammonium chloride through a stomach tube daily. From Wenner's observations it appears that ammonium chloride administered in this way is distinctly helpful in preventing tetany. It also appears efficacious in treatment when tetany has already developed, fully controlling the symptoms manifested within one and one-half hours after administration. After a course of forty days, treatment may be discontinued and the animals placed on a full meat diet without subsequent return of the symptoms. The effect of the ammonium chloride, according to the author, seems to depend on its tendency to raise the blood acidity with, as a result, an elevation in calcium content.

RAPHAEL, Detroit.

THE DIAGNOSTIC VALUE OF DEXTROSE IN THE SPINAL FLUID. S. KATZENELBOGEN and M. STOILOFF, Ann. de méd. **22**:460 (Nov.) 1927.

The value of 60 mg. of dextrose per 1000 cubic centimeters of spinal fluid which is considered as the normal average by French authors is too low. A comparison with other observations and the investigations of the authors points to a physiologic maximum of 90 mg. and a minimum of 40 mg. in each 1000 cubic centimeters of fluid. An increased amount of dextrose, even above 100 mg. is of no differential diagnostic value. It was found not only in encephalitis but also in many other diseases of the central nervous system. A decrease of the amount of dextrose below 40 mg., however, is of important diagnostic value. This decrease was found in most cases of meningitis, in which it always parallels the increase in cells and albumin.

WEIL, New York.

THE METABOLISM OF NERVE FIBERS. EDITORIAL, J. A. M. A. **88**:246 (Jan. 22) 1927.

Nerve cells show symptoms of fatigue and sensitiveness to lack of oxygen as does muscle, but for nerve fibers the evidence is much less convincing. Fifteen years ago, Hill failed to obtain evidence of heat production incident to transmission of nerve impulses. Tashiro has shown, and has been substantiated by Parker, in spite of Bayliss' criticism, that the resting nerve produces 0.00000016 mm. of carbon dioxide. Parker showed an increase for stimulated nerve. The purely physical conception of nerve impulse has ceased to have much weight, and the chemical change is a real factor in nerve transmission.

CHAMBERS, Syracuse, N. Y.

NOTES ON THE MENTAL DEVELOPMENT OF CHILDREN EXHIBITING THE SOMATIC SIGNS OF PUBERTY PRAECOX. LOIE DOE-KULMANN and CALVIN P. STONE, J. Abnorm. Psychol. **22**:291 (Oct.-Dec.) 1927.

A review of available records of sixty-two cases leads to the conclusion that the rate of mental development is normal or subnormal, and seldom, if ever, is accelerated. Specific trends of mental development are probably not closely correlated with specific types of pathologic development or with the possibility that functioning of the endocrine glands underlies the disorder. There is no evidence pointing to a distinctly hereditary basis.

HAMILL, Chicago.

Society Transactions

PHILADELPHIA NEUROLOGICAL SOCIETY

Regular Meeting, Nov. 18, 1927

M. A. Burns, M.D., *President, in the Chair*

DISCUSSION ON EVOLUTION

Evolution and the Darwinian Theory of Human Descent Viewed from the Standpoint of a Multiple Primate Ancestry. Dr. Charles K. Mills.

Before taking up the particular subject set apart for this meeting, namely, evolution and the darwinian theory of human descent, let me turn aside to glance at the last meeting of this society (The Philadelphia Neurological Society, Oct. 28, 1927). I was strongly impressed with the vitality of Philadelphia neurology which this meeting demonstrated. Old but not outworn subjects were newly elucidated, and important original ideas were presented. The cases shown were unusual and well described. The differential diagnosis of cerebral thrombosis and hemorrhage was discussed by contributors who enforced their statements by records of actual work, clinical and pathologic.

The alluring question of the presence or absence of consciousness in apoplectic attacks, thrombotic or hemorrhagic, was placed on a firmer basis than ever before by the actual experiences of men who have given their days and nights to hospital and laboratory work. Originality was exemplified by the paper on thrombosis of the superior cerebral vein, in the study of cerebral toxemia and infections with the production of rheumatic nodules, and in the discussion of the question of lesions of the choroid plexus in their relation to so-called essential epilepsy. To the veterans of this society — to men like Lloyd, Dercum and myself — it is a matter especially gratifying to know that the neurologic work begun in this society forty years ago is in not only a healthful but in an advancing state.

[The rest of this paper appears on page 969 of this issue.]

Early Studies in Morphology of the Brain Which Bear on the Anthropoid Relations of Man. Dr. Francis X. Dercum.

In the early days of the Philadelphia Neurological Society, we were all greatly interested in the theory of evolution which at that time occupied the attention of leading scientists all over the world. We all greedily read the writings of Darwin, Huxley, Spencer and Ernst Haeckel. To these names that of Dr. Joseph Leidy should be added. Dr. Mills early made contributions on the brains of criminals and defectives, while A. J. Parker and myself enjoyed special advantages during our student days. Parker was a special student of Joseph Leidy, while I was a special student of Henry C. Chapman. Chapman was Dr. Leidy's prosector and occupied Leidy's rooms in the building now known as Logan Hall of the University. This gave me access to Dr. Leidy's rooms, Dr. Leidy himself furnishing me with a key. Dr. Chapman had an excellent library on comparative anatomy and histology and also two excellent microscopes, all of which, I need hardly say, added greatly to the happiness of Parker and myself. Chapman was at that time the prosector to the Zoological Garden, and a great mass of most interesting material was sent to the University. Parker and myself gladly aided Dr. Chapman in the studies and dissections that ensued, in which it is unnecessary to add that Dr. Leidy took a lively interest. Among the specimens that were preserved were a number of simian brains and, as I recall, the brains of two chimpanzees. I myself,

became interested especially in the intracranial portion of the sympathetic nervous system and included in my dissections quite a number of the primates. At this time, Parker became much interested in the morphology of the cortex of monkeys and anthropoids, and realizing that his studies outranked in importance those which I was making I readily consented to his having all of the brain material. In this attitude Dr. Henry C. Chapman generously concurred. During my last year at the university, Prof. F. Gurney Smith, professor of physiology, died, and Dr. Chapman was requested to complete the course, which he did brilliantly. It so happened that at that time a group of clinical teachers, headed by Dr. William Pepper and H. C. Wood, were clamoring for revolutionary changes in the faculty, and as the changes threatened — or seemed to threaten — the security of such didactic teachers as Dr. Leidy, Dr. Chapman threw himself into the breach and opposed vigorously changes which seemed to him to be too radical in their nature; as a result, he was not elected to the professorship of physiology. About this time, however, the professor of physiology of the Jefferson Medical College, Dr. James Aitken Meigs, died and Dr. Chapman was triumphantly elected to succeed him. That Dr. Chapman taught most efficiently for many years at the Jefferson College is a matter of history. The fact that I was myself called to the Jefferson years later to join him as a colleague in the Jefferson faculty was to me a source of great pride and joy.

Dr. Parker's studies won for him one of the graduation prizes. His paper was, however, not published for many years. After his death the manuscript fell into my hands and I at once made an effort to secure the necessary funds for the cost of the illustrations. In this I was generously aided by Dr. Mills, Dr. Osler and others. In due course it appeared among the *Proceedings of the Academy of Natural Sciences.* It formed a large volume with many plates.

These events are now between 40 and 50 years old. Dr. Mills, like Parker and myself, and because he was especially interested in cerebral localization, was thoroughly imbued with the importance of studies in the morphology of the cortex. Parker did not take any interest in clinical neurology, but he did take part in our discussions on cerebral morphology and at one time made a joint study with Dr. Mills of a Chinese brain. Dr. Mills made numerous morphologic studies. I studied and made reports on the morphology of the brains of imbeciles, idiots and epileptic persons, the brains of murderers, and I also described three Chinese brains. We early formed the conception of low type brains. We noted in such brains the frequency of unusual confluences of fissures, and we rightly attributed such confluences to failure of the cortical tissue in the given areas to develop to the general cortical level. Of course we noted relative simplicity of frontal, parietal and temporal development from time to time, but our attention after a time became especially directed to the relations between the parietal and occipital lobes. One of the features by which the anthropoid and simian brain generally is characterized is that the relationship between the occipital lobe and the great association areas of the parietal and temporal lobes is much less close than in man. A distinguished French anatomist, Gratiolet, had early called attention to certain convolutions of the human brain which intimately connect the occipital with the parietal lobe, and he called these convolutions folds of passage, plis de passage. Anomalies of the human brain, therefore, in this region came to be regarded by us as of special significance. The parieto-occipital fissure, for instance, in the normal white brain does not extend, except to a limited degree, on the lateral surface of the hemisphere. When it reaches the mesial edge of the hemisphere it is bounded by a well developed convolution which Gratiolet termed the plis de passage supérieure externe. It does not extend on the lateral surface. In the simian brain it not only extends well on the lateral surface but serves as a distinct demarcation between the occipital and parietal lobes. When it was on the lateral surface of the hemisphere, we spoke of it as the external perpendicular fissure, while to the parieto-occipital fissure the name internal perpendicular fissure was applied. I regard the name external perpen-

dicular fissure as preferable to the term sulcus lunatus employed by Elliot Smith. The name "Affenspalte" (ape cleft) employed by some of the German writers is significant. It is also known in the human brain as Wernicke's fissure.

A close relationship between the occipital lobe and the parietal lobe seems to have been the direct result of the change from the quadrupedal to the bipedal attitude. The more that progression became relegated to the posterior or lower extremities the more did the anterior or upper extremities, the arms and hands, become free to grasp, to handle and to examine objects. In the occipital lobes reside not only the function of vision but also such cognate functions as the perception of perspective, of distance, of depth and of the relations of objects in the field of vision to each other. These functions reside in what may be called the visual association areas, whose correct functioning depends on motor centers situated elsewhere, which we need not now consider. Obviously, when an ape form progressed to such an extent that its upper extremities, its hands, were left free to examine objects tactually and to note size, weight, smoothness, roughness, hardness, softness and the other qualities that make up stereognostic perception, it examined the object at the same time with its eyes, and the stimulation of the interplay between the parietal lobe with its great tactual association area and the occipital lobe led to a closer morphologic relationship and to the development of such bridging convolutions as the pli de passage superiéure externe. The failure of a brain to develop the pli de passage supérieure externe is justly to be regarded as an evidence of a developmental failure and therefore of a low type brain. Together with peculiarities of like significance, it was especially pronounced in the brain of one of the murderers that I examined.

The change to the erect posture appears also to have been accompanied by a lessened necessity for the presence of another pli de passage; namely, the pli de passage inférieure interne. This pli de passage allows a free interchange between the mesial surface of the occipital lobe and the callosomarginal gyrus, but in man it has become submerged or is altogether absent.

Time will not permit my dealing with this point in detail, but the fact is so interesting that I cannot refrain from alluding to it. Normally, the parieto-occipital fissure terminates in the calcarine fissure. In an examination of thirty-three brains of negroes, made by A. J. Parker, he discovered two instances in which a confluence did not occur between these fissures. They were separated by a well developed convolution; this convolution Gratiolet termed the pli de passage inférieure interne. In the apes this convolution is constant except in *Hylobates* and *Ateles*, but it is almost uniformly absent in the human brain. (The submergence or loss of the pli de passage inférieure interne in the case of such widely separated forms as the gibbon, an Asiatic anthropoid, and the spider monkey, a South American primate, may fairly well be regarded as an instance of "parallel evolution." It is of extraordinary interest, however, to note that the extreme length of the arms in both forms spontaneously brings about an erect position.) The finding of this convolution in two instances in the brain of the negro is therefore highly significant. In the white brain it was found by Dr. Mills in only one instance, and that was in the case of a murderer. Benedikt of Vienna had also described the brain of a criminal in which the calcarine was separated from the parieto-occipital and put on record also a similar condition which he found in a Chinese brain. I had the privilege of going over the collection of brains of idiots made by Dr. Wilmarth of the Elwyn Institution for Feebleminded Children. Among seventy-five brains, I found four instances of the presence of such a pli de passage. Two of these existed in opposite hemispheres of the same brain. In both instances the convolution was large and well developed. In addition, it was twice found (in the same brain) barely submerged. All of these brains in which the convolution occurred were those of white low grade idiots. The presence of a pli de passage inférieure interne is therefore beyond all doubt evidence of a low type brain.

In the Chinese brain studied by Dr. Mills and Dr. Parker, in those studied by Benedikt of Vienna and in the three that I studied by myself, many peculiarities were noted. Prominent among these were numerous and unusual confluences of fissures. I formed the general conclusion that these facts were expressive of failures of cortical development in given areas. Certainly, such appearances are relatively rare in the white brain. This fact becomes possibly significant when one recalls that similar conditions are frequent in the brains of negroes and largely obtain in the brains of apes. This is especially true of the confluence between the parieto-occipital and the hippocampal, the presence of a deep and well differentiated external perpendicular, and the consequent absence of the various external plis de passage. In noting these features of the Chinese brains, it is important to bear in mind that all of the brains examined were those of Chinese coolies. Doubtless the brains of Chinese of a higher social, that is, of a higher biologic level, would reveal a different series of facts.

It may be interesting to recall the conditions noted in some of the brains of epileptic persons that I studied. The absence of a pli de passage supérieure externe occurred in seven of twelve epileptic brains, and as a result of the confluence of fissures the parieto-occipital fissure became an external perpendicular fissure extending far out on the lateral surface. Many other features of similar significance were noted in other regions of the brain. Taken together with the numerous general morphologic features of epileptics, also repeatedly noted, they were expressive of the general truth of the failure of epileptic persons as a group to reach the level of the average normal organism. It is an amazing and regrettable fact that morphologic studies of the epileptic and insane have gone into the discard.

While it is necessary to be cautious in the inferences to be drawn from the facts which I have here called to mind, one conclusion may be safely drawn: namely, that they are in keeping with the general truth that the organism in the course of its development passes through or rehearses in a measure the various stages through which its ancestors have passed and, second, that when through biologic inferiority or through some pathologic cause the development ceases before the highest level of development is reached, the stage at which the arrest takes place represents, other things being equal, a primitive stage in ancestral evolution.

Exhibition of and Comment on Casts of Fossils of Pithecanthropus Erectus. Dr. Francis X. Dercum.

As is well known, the fossils of *Pithecanthropus* were discovered in 1891 and 1892 by Eugene Dubois, who was at that time a surgeon in the Dutch Colonial Military Service, at Trinil, Java. The fossils, which were believed to have had their origin about 500,000 years ago, were found in a bed of volcanic ash in which there were also a great number of fossils of other extinct animals. The fossils to which the name *Pithecanthropus* came to be applied — a name which had been previously proposed by Ernst Haeckel to designate a "missing link" (at that time still hypothetic) — consist of a calvarium, four teeth including a fragment of a lower jaw, and a left femur. These fragments have been elaborately studied by Dubois and by others, and of late again by McGregor of Columbia University and the American Museum. These remarkable fossils, however, remain perpetually new, and their study is ever rich in new thoughts and interpretations. However, I will confine myself to a mere presentation of the most salient and significant features. The calvarium is much smaller than that of modern man and yet is much larger than that of any of the anthropoid apes. One can see at once that the frontal, parietal and occipital regions are much less prominent, much less developed, than the corresponding regions of the human calvarium; in fact they impress one at once as being definitely less "bulging." Again, the frontal region slopes down rapidly from the bregma and terminates in two large and prominent supra-

orbital ridges, which suggest on the one hand the supra-orbital ridges of the gorilla and on the other those of the Rhodesian man. Compared to the ordinary human skull, these ridges are relatively enormous. Further, running laterally from either side of the occipital protuberance there is a well marked ridge, which evidently existed for the attachment of powerful nuchal muscles, and the slope of the occipital bone is such that the head was carried or "hung" forward, much as the anthropoid apes do, and not equipoised, as in modern man.

When found, the calvarium was filled with a hard deposit, and some time later this was carefully removed by Dubois. In the comparison of a cast of the interior of the calvarium of *Pithecanthropus* and the cast of the brain of a modern man, so-called *Homo sapiens,* one will at once be impressed by the striking difference in size; the brain of modern man is greatly larger than that of *Pithecanthropus.* Dubois estimated the size of the latter as equivalent to 900 cc.; McGregor, as equivalent to 940 cc. That of modern man ranges from 1,300 to 1,600 cc. and much more. That of an Australian bush woman was estimated by Turner as 930 cc. Other instances of brains of less than 1,000 cc. capacity are on record. However, the largest brain of a gorilla, according to Keith, rarely exceeds 600 cc. In size, the brain of *Pithecanthropus* is much nearer to that of man than to the anthropoids.

When one examines the cast of *Pithecanthropus,* one must bear in mind that this is not a cast of the brain itself but merely a cast of the interior of the skull and that in interpretation one must make allowance for the presence of and the space originally occupied by the dura and its vessels, by the cerebrospinal fluid, by the arachnoid and by the pia. One notes at once, in spite of the rôle which these structures must have played in the final result of the process of fossilization, definite cortical landmarks. For instance, one notes in the frontal lobes the outlines of the three typical convolutions and also similar demarcations on the occipital lobes indicating, though less clearly, the convolutions, while on the parietal and other lobes the demarcations are much less defined. In interpreting these results, one should remember that the bones of the vault of the skull are membrane bones, that they are relatively soft and that in the young and growing animal they readily receive and accept the impression of the rapidly developing brain. All who have dealt with the roentgenograms of the skulls of children will recall many pictures of convolutional impressions on the inner table of the skull. Such impressions are not to be regarded as due to abnormal increase of intracranial pressure, or indeed to any other pathologic cause, but are to be regarded as normal and physiologic. In keeping with this one notes that in the frontal region of the cast the convolutions are outlined with a special clearness; doubtless this is due to the fact that the frontal bone is especially impressionable, particularly in the young. This is true to a less extent of the supra-occipital and still less of the parietals.

It is interesting to note that in the cast the sylvian fossa is clearly marked. The fissure of Rolando is only faintly indicated, owing to the dominance of the dura with its vessels. In the occipital region, especially on the right hemisphere, is indicated the external perpendicular fissure (the Affenspalte, the sulcus lunatus). Both the third frontal convolution and the temporal lobe (as far as preserved) justify, from their development, the inference that *Pithecanthropus* had in a measure the faculty of speech.

The teeth are distinctly pithecoid and suggest those of the orang. One observer, indeed, Gerrit S. Miller, looks on them as the teeth of an extinct Javan great ape. Keith, Gregory and others, however, find important similarities to human teeth. Evidently these fossil teeth, like the calvarium and the brain, occupy an intermediate position. The fragment of jaw, which bears a left anterior premolar, shows that *Pithecanthropus* had a vertical chin, which did not project like that of modern man.

The thigh bone is thoroughly human; it is massive but decidedly shorter than the thigh bone of modern man. The large linea aspera which still bears some of the tendinous insertions of muscles leaves no doubt as to the erect posture of *Pithecanthropus.*

These remarkable fossils, as just stated, are ever new, and a number of interesting and important inferences and interpretations, which have not as yet been presented, suggest themselves. However, other and more general statements are, it appears to me, called for. For instance, it is interesting to recall the following well known facts, to which Keith among others directs attention: (1) the forms of fossil man from the older strata are more ape-like than those from the newer; (2) in still older strata, one finds the fossils of great anthropoids — some nine in number; (3) in still more ancient deposits, the remains of small anthropoids; (4) deeper still, no anthropoids, merely monkey-like primates.

All mammalian life appears to have been arboreal in origin. This is clearly true of the primate ancestors of man, who were entirely arboreal. The anthropoid ancestors of man were semiarboreal and semiterrestrial; most of the time was spent in the trees, but they occasionally descended to the earth. This is true of the four living anthropoids and was apparently true of all of the fossil anthropoids with a single remarkable exception. About two years ago, a fossil anthropoid was found which had been purely terrestrial. It was described by Prof. Raymond A. Dart, Professor of Anatomy in the University of Witwaterstrand, Johannesburg, South Africa. It was named by him *Australopithecus.* It had ceased to be an arboreal creature; it had become a cave dweller; it was entirely terrestrial. Lack of food or powerful enemies had driven it out of the tropical forests and across a large open treeless country. This country was, as fossils show, inhabited by enormous carnivora, great cats; self preservation here demanded of this anthropoid the utmost ingenuity and alertness. Safety was at last found in the caves of the limestone cliffs of the Taungs in Bechuanaland.

In the Taungs an enormous and radical change was forced on this anthropoid. All the anthropoids of which there is any knowledge, both the living anthropoids of the present day as well as their progenitors and relatives of bygone times, were fruit eaters. They were not only arboreal or semi-arboreal, but depended for their sustenance entirely on the fruits of the forests and possibly on such nutritious roots as the soil afforded. Under the new surroundings in which *Australopithecus* found himself, there was only one choice between starvation and survival, and this consisted in a change of food; a change of food evidently took place. Hunger permits no hesitation and clearly forced *Australopithecus* to seize and feed on the other living creatures about him. In the fossilized deposits and accumulations in the cave in which the skull was found were hundreds of fragments of bones which had been obviously gnawed; bones of turtles, birds, small insectivores, rodents, baboons and perhaps small bok, and there were also fossilized egg-shells. "The material indicates by its nature, its sparsity, its searched-over character, the careful and thorough picking by an animal which did not live to kill large animals but killed small animals in order to live." Many skulls of baboons had been fractured, doubtless with the aid of a stone, and the nutritious brain material scooped out. No doubt the breaking of other bones gave access to the marrow. From an animal whose ancestors had lived entirely on fruits and vegetables, *Australopithecus* became a carnivorous, or in all probability an omnivorous, animal; for he no doubt found also some vegetable matter, roots, berries, etc., sparse and inadequate as it may have been.

Unlike the gorilla, *Australopithecus* was slight of build and, it would seem, small of muscle. The superciliary ridges, too, were slightly developed; in the gorilla, they are massive. The dentition and jaws were moderate in development and did not suggest an animal that fought with its teeth. The skull revealed a large frontal and, it may be added, also a large occipital development. In other words, in contrast to the gorilla, which in its struggle for existence had developed enormous physical strength, *Australopithecus* had remained physically weak and relatively defenseless; but, on the other hand, his brain under the constant stress of outwitting his enemies and of searching for food, had undergone a development far in excess of that of the other

anthropoids. The discovery of *Australopithecus*, which occurred only a little more than two years ago, has opened up great vistas in the history of both anthropoid and human evolution. Dart estimated the cranial capacity of *Australopithecus* at 625 cc.; he regards *Australopithecus* as intermediate between the chimpanzee and *Pithecanthropus*. After *Pithecanthropus*, one would come to a man at the level of the Piltdown man, *Eoanthropus* (Dawson), then to the Heidelberg man, the Neanderthal man, the Rhodesian man, the Cromagnon man and, finally, modern man. It is not claimed, of course, that all of these forms are ancestrally related to each other; but that they represent stages and periods in the history of human evolution there can be no doubt.

THE RÔLE OF THE CATALYSTS IN EVOLUTION. DR. FRANCIS X. DERCUM.

In his address on "Darwin's Theory of Man's Descent, as it Stands Today," read before the British Association for the Advancement of Science on Aug. 31, 1927, Sir Arthur Keith called attention to the possible rôle of the hormones in evolution. In my address, "On the Dynamic Factor in Evolution," delivered April 28, 1927, at the Inauguration of the Bicentenary of the American Philosophical Society, I expressed myself on this theme as follows: It has long been known that certain substances may induce changes in other substances merely by their presence, they themselves not undergoing any change whatever. For instance, dilute sulphuric acid added to starch will convert the latter into glucose, the acid itself neither qualitatively nor quantitatively undergoing any change. Again, hydrogen peroxide is decomposed by soluble alkalis, also by the mere presence of insoluble substances such as metallic silver or metallic platinum; these substances, as before, do not undergo any change. A similar statement applies to the action of platinum in bringing about the oxidation of alcoholic vapor and to the action of spongy platinum in bringing about the union of hydrogen and oxygen. Similar truths apply to both gold and silver, though neither are as active as platinum. The various substances enumerated, it should be mentioned, may perform the rôle, on the one hand, of reducing agents or on the other, of constructive agents, the chemical bodies resulting being either more simple or more complex in structure. Such action, I need hardly state, is termed catalysis. It implies more especially the setting in motion or the precipitating of possible or impending chemical changes. The action is one in which given substances by their presence alone set into activity chemical processes otherwise dormant without themselves suffering any chemical change. Some facts are especially suggestive; for instance, in the union of hydrogen and oxygen, brought about by the presence of spongy platinum, the enormous increase of surface in the spongy platinum is the immediate causative factor of the union. Such facts as these, so well known, are, I believe, of the utmost importance in interpreting the phenomena of living matter.

It is hardly necessary to call to mind the fact that when a food particle is taken into the interior of an ameba, it gradually disappears and finally becomes part of the substance of the ameba. It would appear that, as in higher animals, the protoplasm of the ameba has the power of fragmenting proteins, fats and carbohydrates. In the higher animals, protein is reduced successively into peptones, amino-acids and a series of intermediate products until the final stage of urea, uric acid and kindred substances is reached. Carbohydrates are by a long series of intermediate stages converted into carbon dioxide and water. Fats, also, are decomposed into glycerin and fatty acids and by a further reduction into simpler bodies until end-products are likewise reached. During these changes, energy is released. There is no inherent improbability in the inference that changes like or analogous to these take place in the ameba. Certain it is that the various food particles are so metamorphosed that they disappear into the general substance of the organism and after a time can no longer be distinguished. Evidently they have added to the substance of the ameba. Facts and inference both justify the statement

that physical increase of size and final separation or division into two bodies follow the continued ingestion of food; and this constitutes the second fact of importance, for it means that *the constructive or upbuilding change far exceeds the reducing change.* That which is observed in the ameba, it is needless to say, is true of other living forms and, to state it in other and more familiar words, living protoplasm is the seat at one and the same time of both anabolic and catabolic processes, of both an upbuilding and a down building change. The fact that the upbuilding process exceeds the down building process is one of great significance. Growth and multiplication both have their explanation in this fact. Various terms, it need hardly be added, have come to be applied to the substances instrumental in inducing these changes, such as enzymes, ferments and hormones, but the general term catalysts can be conveniently applied to them all; and the inference further is justified by the facts at our command that their action in no way differs from that of catalysts of the non-living world, for like the latter they act merely by their presence. How intensive this action may be is illustrated by the fact that under the influence of enzymes or ferments, chemical changes take place in living organisms which can only be brought about in the laboratory by powerful reagents and high temperatures. For example, the change of protein to amino-acids is effected in the laboratory by boiling in concentrated hydrochloric acid. In the organism it takes place at an equal rate at ordinary temperature and in a medium which is only just faintly alkaline or neutral (Bayliss: Principles of General Physiology, ed. 3, 1926, p. 301).

With these conceptions of catalytic action, both in the non-living and the living world, one may consider the possibilities of the origin of living matter; namely, as the result of gradual changes taking place and extending over vast periods of time and resulting from the interaction, combination and association of previously existing substances and in conformity with natural laws. In all probability, the complex substances that were thus formed consisted at first of exceedingly minute molecular aggregations. These resulted, doubtless, from the synthesis of less complex bodies. The very fact of such a synthesis implies the early action of catalysis on the part of the intermediate and primitive bodies, bodies which today no longer have existence on the earth. Little by little these aggregates became more and more complex and gradually and in an increasing degree manifested properties which one to-day associates with living matter. At first these aggregates were without definite structure, unless one considers their possible molecular arrangements as structure. They were also without definite form, though it is safe to infer that they were rounded bodies. Little by little and in increasing degree, they manifested properties which finally were frankly those of living matter; namely, constant chemical interchange with the environment, constant increase and constant diminution in substance, all of these phenomena being inextricably interlinked with and dependent on the ever increasing catalytic power of the aggregate. The living protoplasm of today has become the complex aggregate that it is because of the catalytic power of its primitive proteins to force other materials, often of vastly different chemical structure, into association with itself. "Life," it may be assumed, has thus made its appearance. It may now be traced further.

According to the views formulated by the biochemist of McGill University, Prof. A. B. Macallum (*Physiol. Rev.*, April 1926, vol. 2, no. 2), life originated in the ocean water of the archean period, and the earliest organisms must have been of the micellar or ultramicroscopic kind. Later, these micellar organisms gave rise to multimicellar aggregates and still later these gave rise to simple and undifferentiated cellular forms from which in turn others evolved which were less simple and approximated in complexity the simplest living organisms of today. Further, these organisms synthesized their own constituents from the available nitrogen and carbon dioxide from the air, the sulphur from the sulphates, the phosphorus from the phosphates and the iron from the sea water. There was as yet no nucleus.

This interpretation by Macallum seems to me to bear within it the inherent probability of truth. It is a legitimate inference, further, that the synthesis outlined by Macallum resulted in the formation of amino-acids and the association of the latter into groups to form proteins. Probably the formation of proteins occurred long before the cellular stage was reached, indeed, at the time the first molecular aggregations made their appearance.

After the formation of proteins, the next important step was without doubt the differentiation of nucleoproteins and their grouping together to form the nucleus of the cell. According to Macallum, the contents of the normal cell nucleus does not know the inorganic world. "It contains not a trace of potassium, or of chlorides, phosphates, carbonates or sulphates, and accordingly no sodium, calcium or magnesium, although these four elements are found in the cytoplasm." When one considers the remarkable rôle which the nucleus plays in the process of cell division, it suggests at once that the nucleus dominates the cytoplasm. In other words, while the cell contents as a whole probably possess catalytic powers in varying degrees, the dominating catalytic power resides in the nucleus. Judging from the facts, it would appear that the ions of the alkaline and earthy bases are automatically excluded from the nuclear material. Possibly to this exclusion is due this dominating catalytic power of the nucleoproteins. It would appear that the presence of crystaloids would tend to inhibit this catalytic power.

A protoplasmic mass with differentiated nuclear material having once made its appearance, a further differentiation next took place. Two protoplasmic aggregates coming into contact or within the range of physical or chemical, or shall one say of catalytic, reaction with each other, the mass possessing the greatest catalytic power would dominate the second; i. e., it would cause the second to be so changed by the greater catalytic power of the first as to be capable of being taken into and finally incorporated with the substance of the latter, the latter possessing the more intensive anabolic processes. To state it in physiologic terms, the second mass would be digested and assimilated by the first. Herein, I believe, lies the key to the differentiation of living protoplasm into animal and vegetable forms. This "cannibalism," early established, was followed later by a lessened and finally by a loss of the power of the dominant form to construct its proteins and other constituents directly from the materials of the environment. Here, then, is a forward step of great importance in the progress of evolution. Herein also, I believe, lies the explanation of the disappearance of the intermediate and primitive bodies which preceded the appearance of fully developed protoplasm. In the "catalytic struggle" they failed to survive and under changing conditions have long since ceased to be formed.

The fact that in living protoplasm the building-up or anabolic processes immeasurably exceed the building-down or catabolic processes is of vast importance. As a result, living protoplasm has spread all over the earth; it has not only invaded sea, plain, valley and mountain, but has penetrated into every corner, nook and cranny of earth, water and air in which the conditions necessary to its chemical and catalytic interchanges exist. All lacunae are filled. Here is the explanation of its survival and persistence through such vast periods of geologic time. Further, living protoplasm is a plastic aggregate which has been able to adapt itself to the most varied environmental conditions. It certainly does not seem strange that it has assumed the most varied forms. I, myself, believe that in the marvellous catalytic power of the protoplasmic aggregate, that power in which the anabolic processes so greatly exceed the catabolic processes, lies the key to the explanation in a large measure of evolution.

Time will not permit of the application of this dynamic principle to the details of the evolution of animal and vegetable forms. One must be content here with the general conception that further and increasing differentiations are dependent, first, on the materials available in the environment and, second, on the innate catalytic power of the living protoplasm. Necessarily, under

given conditions, there ensued an increasing complexity of the chemical changes in the latter. In unicellular forms, as in the ameba, these changes remained relatively simple; not so, however, in the metazoa. Here one finds that each cell possesses not only the special structure that enables it to fulfil the functions of the tissue of which it is a part but also special ferments by means of which it builds itself up and adds to its own substance out of the general material of the blood plasma. Each cell possesses the power of taking in foreign materials, of fragmenting them, and of utilizing them for purposes of reconstruction or as sources of energy. In return, each cell gives up to the blood stream such substances as are of no further value to it. These substances, the products of its continued chemical changes, may consist in part of materials so far reduced that they are no longer sources of energy and are ready for discharge from the organism as waste materials, such as urea; or they may consist of substances which have, first, still a food or, second, a catalytic, value for the cells of other tissues. As an instance of the first may be mentioned the glycogen of the liver; as an instance of the second, the epinephrine of the suprarenals.

It is important to envisage continuously the organism as a whole and to bear in mind that all of the tissues of the organism play a rôle in its chemical changes; i. e., in its metabolism. What has been learned of the various glands of internal secretion as to their embryology and their morphology justifies the statement that certain glandular structures — perhaps like the thyroid early provided with a duct and serving at first certain ulterior purposes — became the seat of the formation of intensive catalysts serving certain special functions. Certain structures, most diverse in origin and probably serving other functions early in the evolution of the organism, became associated in the general and special metabolism of the organism. Thus the thyroid, the pituitary, the chromaffin system and the sympathetic nervous system became synergically related; they constitute a synergic group. While certain of these, like the thyroid and pituitary, came to influence especially growth and development, the skeleton and other structures, all of them came to act together to promote activity of the tissues; all of them increase metabolism; all of them promote catabolism and the release of energy. On the other hand, certain other structures have to do with the storing up of energy. The pancreas and, in fact, all the glands of the alimentary tract together with its adnexa are under the influence of that portion of the sympathetic nervous system spoken of as autonomic. They are all concerned in the processes of digestion and assimilation, in other words, in the storing up of energy. The thymus and parathyroid are both in synergic relation with the pancreas and must therefore be added to this group. All are opposed to the disintegration, to the downward change of the body constituents, in other words, to catabolism.

To repeat the facts in other words, the symbiosis of the cells in metazoa leads gradually to the differentiation of the glands of internal secretion. This meant, in an increasing degree, specializations of the catalytic processes of living protoplasm. This implied further, in a large measure, the specialization of chemical changes in the individual cells, as well as a mutual inhibition of the chemical processes in the organism in its entirety; and this is exactly what occurred.

Almost every one who has thought seriously on the subject of evolution and who has realized the truth, i. e., the fact of evolution, has attempted some explanation of its cause — has either framed some explanation to satisfy himself or has sought refuge in the explanations framed by some of the world's great thinkers. Two names, of course, stand forth in striking prominence, that of Lamarck and that of Darwin; the one deals with the rôle which the use of organs may have in their development; the second points out the rôle which nature plays in the struggle for existence and in the survival of the fittest, a fact which has been stated obversely as the death of the unfit. Both explanations lack a primal, driving cause inherent in the living organism itself. Both enter into the explanation of results but not as to origins. Bergson

(Creative Evolution, translated by Arthur Mitchell, Ph.D., 1911), feeling the necessity of a special cause, used the expression the "vital impetus" (élan vital); this, however, merely assumes a fact without offering an explanation as to the origin of the fact; Eldridge used the term "organizatory factors" (The Organization of Life, 1925) which equally fails to explain, while the Italian physiologist, Giulio Fano, satisfied himself with the expression the "cosmic will." For me the term, the "cosmic will" — and I say this with all courtesy — has nothing but the vaguest significance. Like the other terms, it does not give any help in the solution of the problem which evolution presents.

Is the conception of a dynamic factor of any value in the solution of this problem? Of the existence of such a dynamic factor there can be no doubt. The enormous catalytic, constructive power of living protoplasm is one of nature's outstanding facts. Obviously, it must be admitted into our problem. Is it of itself sufficient to explain the differentiations and increasing complexities of organisms? I believe that it is. I believe that, taken in connection with certain other factors which likewise have to do with the reaction of the organism to the environment, it is the one outstanding cause of evolution. The metabolism of the metazoa, complex as this is in the higher forms, cannot be a fixed, an unchanging metabolism. The metabolism of the organism must, of course, within certain limits, vary with its environment. The cells of the various tissues of the metazoa have the power of appropriating and assimilating certain materials from the blood plasma, but clearly in the exercise of this function they are dependent on the materials presented by the blood plasma. The blood plasma in turn must build itself up out of such materials as come to hand and are ingested by the animal. It follows that if given variations in the environment occur, e. g., in the food, these variations must affect the constitution of the blood plasma, and thus the metabolism of the entire organism. From such evidence as presents itself it is safe to infer that, in the past, variations in the environment, food, character of medium, water or air, temperature and other climatic conditions, took place slowly and allowed of adaptations of the organism to these gradually changing conditions. It can readily be comprehended that such a modified metabolism could be transmitted to the offspring for it would be a general character, one affecting the organism as a whole. Indeed, the question of the heredity of such a modified metabolism need not be considered, if one reflects that the young offspring would necessarily be submitted to the same modified environment and would biochemically adapt themselves to it just as had their predecessors, perhaps even more readily, because the young are relatively more plastic. With this interpretation in mind, one may admit and dispense with the discontinuity of variations so much insisted on by Bateson, for the reason that spontaneous variations, so-called, are really not a part of the problem. Further, it would appear that increasing complexity and differentiation in response to environmental change can go on almost indefinitely so long as previous adaptations to the environment have not imposed on the organism such fixations or limitations of structure as to make further adaptations to changing conditions impossible. And yet the existence of such highly modified mammals as the seals and cetaceans, the manatee and the dugong, not to speak of that marvellous aquatic reptile of bygone days, the ichthyosaurus, show how much adaptation is possible even in forms already highly differentiated. Probably one should admit here the play of the theory of Lamarck, bearing in mind that the use or overuse of an organ must in its turn react on the metabolism of the animal and doubtless in many cases react directly on given glands of internal secretion. When one reflects again on the rôle of the pituitary and thyroid in growth and skeletal development, the facts become more than suggestive. Suggestive, too, are such facts as the appearance and disappearance of the notochord in ascidians. It would seem that the to and fro movements of the young in swimming would lead spontaneously to the deposit of gelatinous or other fixative materials in the only portion of the organism relatively at rest and capable of furnishing a point of fixation for muscular action; while in the

subsequent sessile condition, the notochord thus formed would again spontaneously disappear from nonuse. Lamarckism should, I believe, be given its due weight. This is true also, of course, of the great static, inhibiting principle enunciated by Darwin. Each has its rôle to play in interpretation. Professor Jennings, of Johns Hopkins University, lays great stress on the influence of changed conditions of the environment and, in his book "Prometheus," uses among other things as an illustration the well-known facts of the transition of the aquatic form of axolotl to a terrestrial form (Prometheus or Biology and the Advancement of Man, New York, E. P. Dutton & Co., 1925, pp. 45 to 47).

I am not presenting here a theory of evolution. I am merely calling attention to a dynamic, driving principle inherent in living matter. Further, the increase in mass leads to a division of the mass, i. e., to multiplication or reproduction. I believe that the same factors enter into this division whether the division is expressed by simple fission or by the more complex process of mitosis; namely, by the interactions, catalytic and chemical, of the contained colloids and crystalloids. Conjugation and sex differentiation also find an explanation here, an explanation already hinted at in the view presented earlier in explanation of the differentiation of animal and vegetable forms. The problems of heredity follow a similar course. The conceptions here presented suggest, among other things, the daring thought that the theory of genes is unnecessary. The chromosomes are aggregates of amino-acids or proteins which are the seat of continuous and progressive catalytic and chemical changes. They can react only with the other cell contents and with each other, in certain definite ways. The changes which take place are necessarily consecutive. They give rise, finally, in the offspring, to certain resultant qualities which are the same or similar to those presented by the parent form. This view seems to me preferable to one that assumes the existence in the chromosomes of thousands of minute chemical parcels which contain the materials necessary for the transmission of the various hereditary qualities. In keeping with this thought, Professor Jennings expresses himself as follows: "The characteristics of the adult are no more present in the germ cells than are those of an automobile in the metallic ores out of which it is ultimately manufactured" (Prometheus or Biology and the Advancement of Man, p. 28).

Explanation of the Lantern Views Illustrating the Papers of Drs. Mills and Dercum. Dr. Charles K. Mills.

A few pictures were shown which assisted in illustrating the points of view which have led to the adoption of the theory of a multiple primate ancestry for the human race. It will be recalled that Dr. Mills described the appearances and departures from the standard or normal human brain of the brains of criminals, imbeciles, paranoiacs and also the brains of the negro, the Chinaman and different types of European people.

Figure 1 shows the brains of primates from the lemur to man arranged in phyletic series from Edward Anthony Spitzka's book "A Study of the Brains of Six Eminent Scientists and Scholars belonging to the American Anthropometric Society," etc.

One extremely good photographic representation was shown of a negro brain, viewed from above, taken from Parker's work on the "Morphology of the Cerebral Convolutions with Special Reference to the Order of Primates."

On the screen two views of the brain of a Chinaman were also shown, the case being one reported jointly by Dr. Parker and Dr. Mills. This brain had some features not usually found in the Caucasian human brain. In both the right and the left hemisphere of this brain, bridging convolutions occurred in the calcarine region, a condition not usually found in the white race. In man, as Parker pointed out, the occipital lobe reaches its highest development, as it is also the last to develop completely.

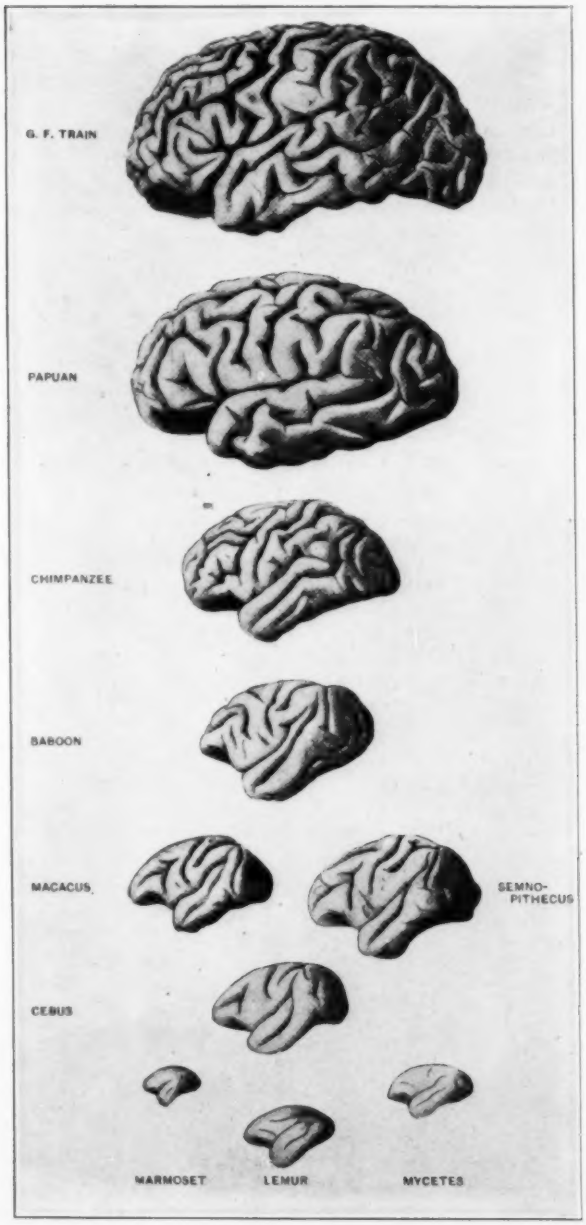

Fig. 1.—Brains of primates from the lemur to man arranged in phyletic series. Taken from "A Study of the Brains of Six Eminent Scientists and Scholars Belonging to the American Anthropometric Society" by Spitzka.

Fig. 2.—Darwin's home, Downe House, in Kent.

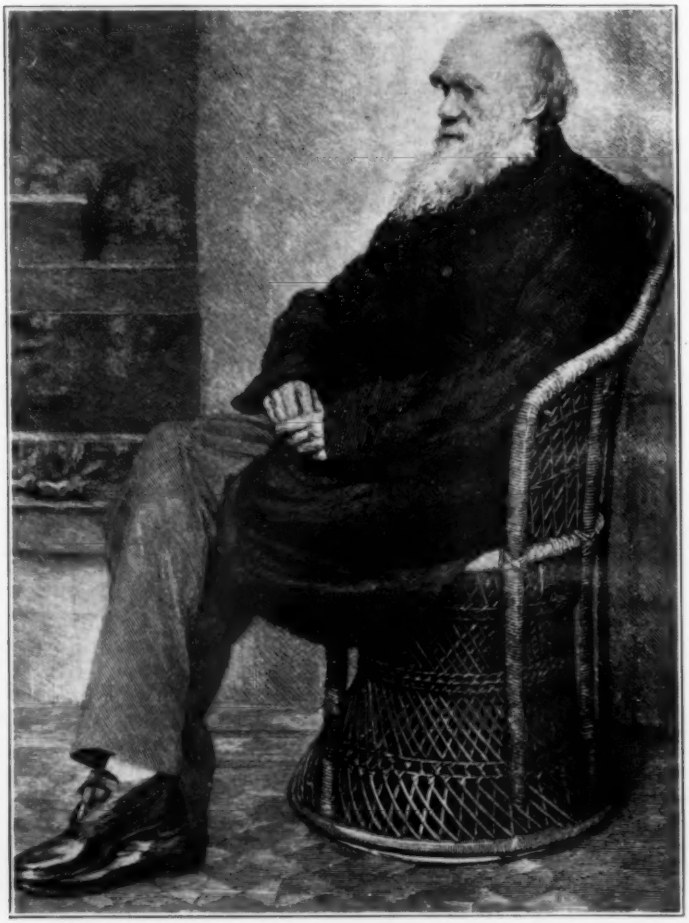

Fig. 3.—Photograph of Charles Darwin.

Two pictures were shown of the brain of a confirmed murderer and criminal, Joseph Taylor. The brain showed many simian appearances and reversions.

The next picture, taken from Spitzka, was a dorsal view of the brain of Gauss, the mathematician. This showed great fulness and complexity of the fissures and convolutions.

The two illustrations show the lateral and mesial aspects of the brain of Joseph Leidy, from Spitzka's book. Even a casual study of these pictures show how the brain of Leidy, like that of Gauss, differs in its surface morphology from the racial and low type brains just described.

The third illustration gives a view of Darwin's home "Downe House" (fig. 2) in Kent, England, and a photograph of Charles Darwin (fig. 3) taken from "The Life and Letters of Charles Darwin" by his son, Francis Darwin.

SUGGESTIONS TO MAKE THE HOME OF DARWIN A NATIONAL SHRINE.

In his presidential address at Leeds, Aug. 31, 1927, Sir Arthur Keith made an appeal for a fund to purchase Darwin's home at Downe in Kent, so that it might be preserved for the nation. Mr. George B. Browne, a retired surgeon, on reading the appeal at once telegraphed to Sir Arthur, offering to make himself wholly responsible for the gift. The cost, with some endowment fund, is estimated at from $60,000 to $75,000. Mr. Browne made it a condition that no other contributor is to be asked to share the cost with him.

Mr. Browne was admitted to the membership of the College of Surgeons in 1874, and for fourteen years acted as assistant to Sir Henry Thompson. He is an antiquarian and an enthusiastic collector.

In offering to buy Downe House and to establish a fund for its perpetual upkeep, Mr. Browne is giving expression to his profound admiration for the work of the great naturalist. He considers that the house in which evolution was cradled should be as reverently preserved as Shakespeare's birthplace. He desires that the house should be restored as nearly as possible to its condition when Darwin lived there. When the house and garden have been restored, Mr. Browne wishes them to be opened without charge, to visitors who could then be shown Darwin's study, laboratory and living-rooms much as when he left them. He also expressed the wish that some physician of slender means and good record should be appointed the custodian.

Sir Arthur Keith has suggested that out of the endowment fund, money should be spared for a prize to be given every second year for the best contribution to biologic knowledge. Downe House is the property of Darwin's son, Prof. Francis Darwin, and is now a school.

LANTERN SLIDE DEMONSTRATION OF A SERIES OF PRIMATE BRAINS. DR. WILLIAM B. CADWALADER.

Sir Arthur Keith, in a lecture given at Manchester, England, in August, 1927 (*Brit. M. J.* **2**:441 [Sept. 10] 1927), quoted Prof. Elliot Smith, who summarized the results of his extensive anatomic studies as follows: "No structure found in the brain of an ape is lacking in the human brain, and, on the other hand, the human brain reveals no formation *of any sort* that is not present in the brain of the gorilla or chimpanzee. The only distinctive feature of the human brain is a quantitative one."

It is generally agreed that the brain of an anthropoid ape is surprisingly similar to that of man. In all apes each cerebral hemisphere (fig. 1) is divided into the same lobes as in man; that is to say, frontal, parietal, occipital and temporal. The cerebrum does not completely cover the cerebellum. The frontal and parietal lobes are separated by the central fissure. The arrangements of the motor centers is the same as that in man. This has been demonstrated experimentally by Sherrington and Grünbaum.

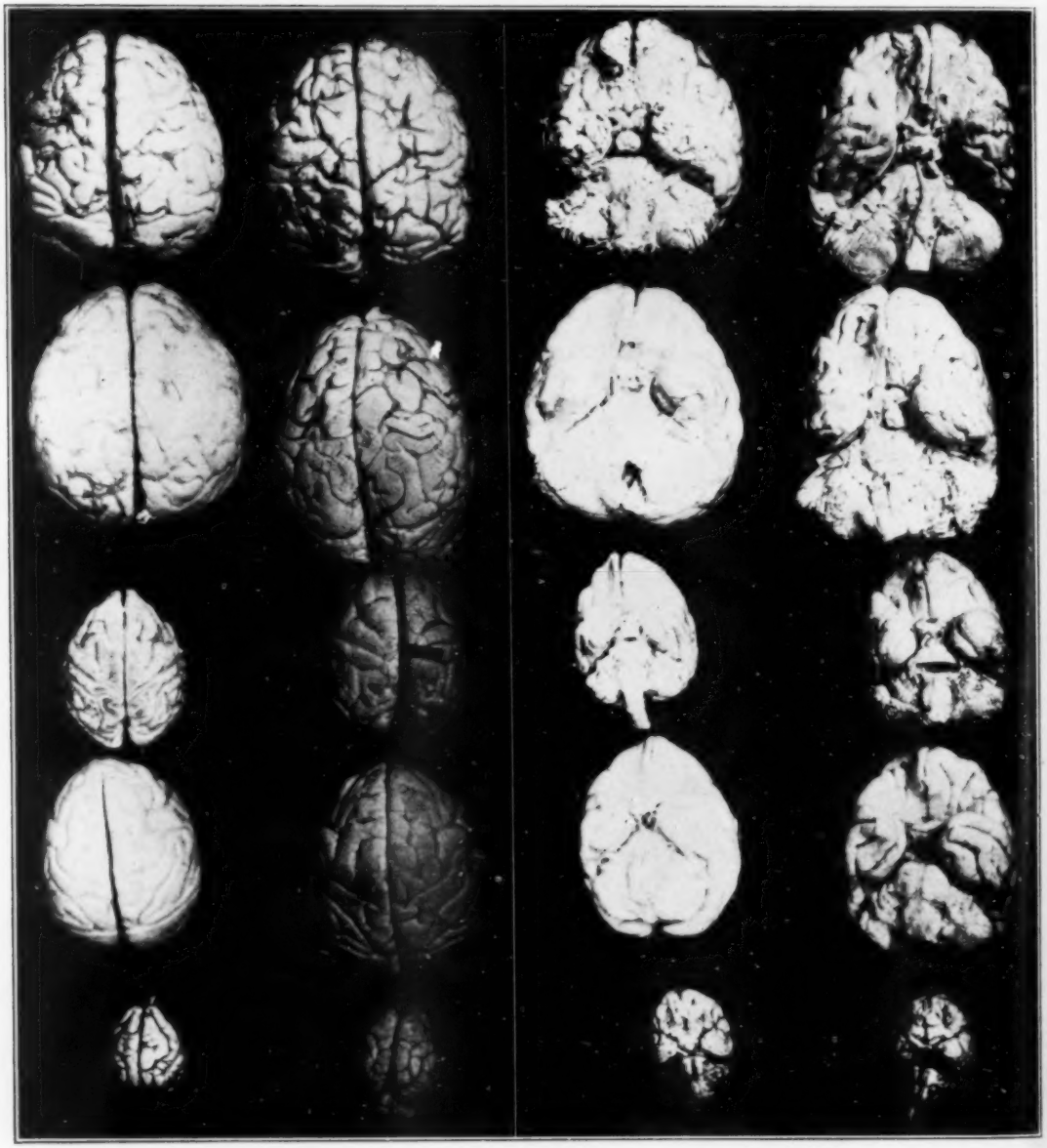

Figure 1 Figure 2

Fig. 1.—Superior view of brains of primates, ranging in size from that of a chimpanzee to that of a lemur.

Fig. 2.—Inferior view of same brains shown in figure 1.

The sylvian fissure is complete (fig. 2) only in man when the anterior and posterior limbs are present. In apes, the anterior may be absent and the posterior may be bifid. Apes have a simian "sulcus" or "Affenspalte" which sweeps across the hemispheres and lies behind the parieto-occipital sulcus, or may pass into it. In man, the small sulcus lunatus corresponds to the Affenspalte.

The following summary is given by Sonntag (Morphology and Evolution of Apes and Man, p. 278): The human brain differs from that of apes in the following ways: (1) it is larger both absolutely and relatively to the size of the body; (2) the neopallium is larger and the association areas are more complex; (3) the parietal and frontal lobes are more voluminous; (4) the sylvian fissure is complete and the insula is concealed; (5) there are calcarine and retrocalcarine sulci; (6) the small sulcus lunatus corresponds to the large Affenspalte; (7) secondary sulci are more numerous; (8) the cerebrum conceals the cerebellum more completely, and the latter shows a higher phase of evolution; (9) many greater complexities are the result of a large neopallium; (10) some of the cranial nerves are smaller; (11) the corpus callosum is larger.

A lantern demonstration was given of the brains of the following primates, as shown in figures 1 and 2, superior and inferior views, from above downward in the order shown in the accompanying table.

Weights of Animals and of Their Brains Shown in Figures 1 and 2

	Weight of Animal (Kg.)	Weight of Brain (Gm.)
Chimpanzee (*Pan niger*)	56.8	300
Chimpanzee (*Pan niger*)	32.7	430
Orang (*Simia satyrus*)	18.6	400
Orang (*Simia satyrus*)	19.5	350
Hainan gibbon (*Hylobates hainanus*)	6.0	115
Silver gibbon (*Hylobates leuciscus*)	3.03	75
Rhodesian baboon (*Papio rhodesia*)	8.0	220
Guinea baboon (*Papio sphinx*)	6.8	180
Black and white lemur (*Lemur varius*)	2.5	24
Ring-tailed lemur (*Lemur catta*)	1.7	30

A slide which had been made from an illustration (fig. 3) in Sir Arthur Keith's book (The Antiquity of Man, vol. 2, p. 612) was shown, illustrating a preparation of the reconstruction of the brain of the Gibraltar man, the name given to one of the prehistoric skulls of the Neanderthal race, which he believed belonged to a man that had existed in the pleistocene geologic period, computed by him to have dated somewhere between 100,000 and 200,000 B. C. Sir Arthur Keith selected this particular skull from a collection because he thought it represented one of the smallest of any of that race, and for this and other reasons he believed it more nearly approached the anthropoid type. Superimposed on the outline of this brain is placed the brain of a young gorilla, the ape believed to be nearest to man. The diagram shows distinctly the great expansion of the reconstructed Gibraltar brain, its comparison to the brain of the gorilla, in parietal and frontal regions; and it shows most especially the development of the inferior frontal convolution in the human type, with the corresponding lack of development of this portion of the frontal lobe in the brain of the gorilla. Such differences have been emphasized from time to time by different authorities, and have been used as an argument to explain the absence of the function of speech in apes. This lack of function of speech in turn has been regarded as a factor in explaining the enormous psychologic gulf that exists between man and apes.

If one classifies man by strict zoologic methods, as seems justified, then he must be placed in the order of primates together with the others of this order. But it should not be forgotten that zoologic classification is based on strict comparison of anatomic structures. It does not make any allowance whatever for the enormous psychologic differences existing between the highest and lower primates.

DISCUSSION OF PAPERS PRESENTED BY DRS. DERCUM AND MILLS

DR. J. HENDRIE LLOYD: Dr. Mills' paper contained many suggestive ideas, but the one that interested me most was the suggestion of the multiple origin of the human race. This had once been held to be a sort of heresy by certain evolutionists. Their idea was that the human race was all of one stock, descended, as it were, from a single root and represented in a common ancestry. Their favorite illustration was a genealogic tree, showing diagrammatically the roots fixed in the eocene lemnes, then the trunk ascending and giving off branches, first of the anthropoids, then successively of the various

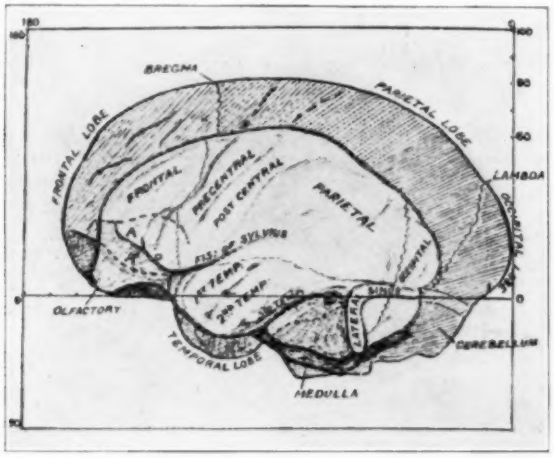

Fig. 3.—Profile drawing of the cast of the brain from the skull of a young gorilla superimposed on a corresponding drawing of the cast from the Gibraltar skull. The Gibraltar cast exceeds that of the gorilla in all dimensions. From "The Antiquity of Man," by Sir Arthur Keith.

kinds of men, until at the very top was seated *Homo sapiens* or modern man. The objection to such a scheme is that it sidetracks all lower forms of primates and tends to deny that they are in the line of human ancestors. This is clearly shown in such recent works as those of Keith and McCurdy. This school of evolutionists seem unwilling to recognize any transitional types as they are found, although this is the very thing that the doctrine of evolution requires. When, for instance, the Piltdown man was discovered, one anthropologist objected to the shape of the lower jaw, and on this slender basis denied that the Piltdown man could be a human ancestor. The lower jaw was too simian, although the upper part of the skull was much better. To an unprejudiced observer this looks like a good transitional form, combining both human and anthropoid characteristics, and so thought Elliot Smith, if I remember correctly.

The doctrine of a single human stock might almost be called the Garden of Eden theory. It requires one to believe that the human race has been evolved from some one spot on the earth's surface, although just where that

spot was no one can tell. Only recently an expedition went to the interior of Asia to find the original home of the race, but it brought back only a dinosaur's egg. Griffith Taylor also believed the race took origin in Central Asia, and thence spread by successive "waves" over the earth, beginning with the negritos, like a volcano erupting successive streams of lava. In fact, Taylor called it the "Lava Flow" theory. It is such fantasies that tend to bring the doctrine of evolution into disrepute.

McCurdy said that the stem took root at the beginning of the tertiary period (eocene) in North America. These early lemurs resemble lemurs still living in the Malay Archipelago; but he quoted Matthews who believes the center of lemur dispersal to have been in Asia north of the Himalaya Mountains. Cope believed that the *Anaptomorphus* was the common ancestor of apes and men; it is found in the eocene period of North America; another thought the earliest were the lemuroids of Western Europe. How are all these conflicting data and opposing views to be reconciled with the doctrine that the human race is of only one stock? It would seem more in accord with the evidence to suppose with Dr. Mills that there has been more than one stock. This was also Klaatsche's idea, who believed in at least two distinct origins of man—one in Africa, which included the gorilla and Neanderthal man, and the other in Asia, including the orang and possibly some modern men. But if there were two, why not more than two? The processes of evolution have been going on everywhere and are not limited to one region. Like causes produce like effects.

Keith believed that the Neanderthals, who lived until a comparatively recent day, were a distinct genus, not closely related to *Homo sapiens*. Perhaps they were not related at all. On the other hand, Hrdlika maintained recently that the Neanderthals were in the direct ancestry of modern man. Thus opinions differ.

Dr. Dercum's paper seems to reflect a tendency that has been apparent for sometime among evolutionists. It is a movement away from descriptive morphology toward the domain of physicochemistry. Osborn, for instance, dissatisfied with Darwinism, tried to evolve a theory to account for variation. This question was avoided by Darwin, Huxley and the older evolutionists. Darwin simply accepted the facts of variation and built his doctrine on those facts. He expressly disclaimed any knowledge of laws and processes underlying those facts. The more recent schools concern themselves with the more occult problems of organic matter. They believe there is some force or energy which impels and controls organic matter in processes of evolution. They use such terms as "internal urge," and the "élan vital" of Bergson. These are obscure terms to explain this organic force in terms of physicochemistry.

Osborn said: "Germ evolution is the most incomprehensible phenomenon which has yet been discovered in the universe." This opinion will probably be shared by most reflecting men. Take, for instance, the embryo. The embryo develops an eye which has never seen the light, a stomach which has never digested food, limbs which have never walked, lungs which have never breathed. All this is certainly not to be explained by any law of the adaptation of the organism to the environment. It is due to an inherent, mysterious, unknown power in the germ plasm. Osborn's statement is not an exaggeration; this is the most incomprehensible problem which nature has set before us. Whether it will ever be solved is a question.

DR. J. PARSONS SCHAEFFER: I want to assure Dr. Mills and Dr. Dercum that I have never been offended by the doctrine of evolution; rather I have thought of it as a noble way of bringing man to his present estate. So far as scientific facts can be brought to bear on man's history, they point in the direction of man's evolution from a more lowly form or forms rather than his appearance by a special act of creation. While there may be differences of opinion as to details, there appears little doubt on the more basic problems involved.

Dr. Dercum's second paper has to do with the "beginnings of things." One cannot help but recall the statement of Smuts in this connection, which is worthy of thoughtful reading: "The acceptance of Evolution as a fact, the origin of life-structures from the inorganic, must mean a complete revolution in our idea of matter. If matter holds the promise and potency of life and mind it is no longer the old matter of physical materialists. We have accepted Evolution but have failed to make the fundamental readjustment in our views which that acceptance involves. The old mechanical view-points persist and Natural Selection itself has come to be looked upon as a mere mechanical factor. But this is wrong. Sexual Selection is admittedly a psychical factor and even Natural Selection has merely the appearance of a mechanical process, because it is viewed as a statistical average from which the real character of struggle among the living has been eliminated.

"Nineteenth century science went wrong mostly because of the hard and narrow concept of causation which dominated it. It was a fixed dogma that there could be no more in the effect than there was in the cause; hence creativeness and real progress became impossible."

Reference has been made to Sir Arthur Keith. In his Leeds address (1927), Sir Arthur pointedly stated that Darwin was and still is right, that man under the action of biologic forces which can be observed and measured has been raised from a place among anthropoid apes to that which he now occupies.

I am not certain, as someone has suggested, that Keith is committed to the "one line" origin of man. In the address referred to, Keith, among other things, said: "Our older and discarded conception of man's transformation was depicted in that well known diagram which showed a single file of skeletons, the gibbon at one end and man at the other . . . . We should never have made this initial mistake if we had remembered that the guide to the world of the past is the world of the present. In our time man is represented not by one, but by many and diverse races . . . To unravel man's pedigree, we have to thread our way, not along the links of a chain, but through the meshes of a complicated network."

This does not appear to oppose the thought of a multiple primate ancestry for man.

Reference has also been made to Professor Osborn, who has brought profound knowledge, insight and industry to bear on the problems of the evening. I fear he is misunderstood. It is not that Osborn does not believe in the evolution of man from lower forms, but he has come to the conclusion that apes have no close physical and mental kinship with man. He considers the idea of man's ape ancestry faulty, and this largely due to our previous ignorance of the real course of human evolution.

Osborn, citing Gregory (*Science*, vol. 65), arrived at the following striking conclusions: 1. The human family, like other families, includes a great number of independently evolving phyla which for untold ages evolved independently of each other in different parts of the world. 2. As in other mammals, one shall rarely find the true stem forms at the bottom of the phyletic lines. 3. The older of these human phyletic lines may well run back as far as many other lines of mammals do, that is, at least to the Lower Oligocene or Eocene. Hence even in those inconceivably remote ages one may expect to find Dawn men — erect-walking, plains-living, large-brained, speaking men, totally dissociated from the family of the apes, especially in their evolutionary trend which was toward life in the open, while that of the apes was toward more and more specialized life in the trees. 4. Hence the idea of man's ape ancestry is a myth and a bogie, due to the previous ignorance of the real course of human evolution. For millions of years man has been a ground-living, erect-walking being and if at some still earlier period he may have passed through an arboreal stage, such a stage could not have been long or have left a deep imprint on his skeleton and nervous system.

Gregory regretted that he could not follow his "honored leader into the new and spacious field of thought" and deemed it his "duty to defend the old and always unpopular view of Darwin, Huxley, Haeckel and others of previous generations." He said: "In conclusion then, during the past twenty years I have published a series of investigations on the classification and evolution of the vertebrates, dealing since 1916 especially with the origin of man, and during this period I have been unable to discover a single valid objection to the direct evidence afforded by comparative anatomy and in harmony with the paleontological record of the entire Primate order, so far as known, namely, that man's relatively close kinship with the chimpanzee and the gorilla is an unassailable fact. . . .

"Refusing to accept even the paleontological record so far as it is known, disregarding the cogent and direct evidence of comparative anatomy, many paleontologists do not hesitate to extend to man supposed laws of evolution deduced from the study of orders of mammals which in their entire organization and history stand in wide contrast to the primates. From such analogies has been conjured the Eocene Dawn Man — a colossal anachronism some forty million years ahead of his time in the world's history."

Osborn and his pupil Gregory, therefore, appeared to be at the opposite ends of the log. They sighted "the same vastly distant and obscure event, the emergence of man, from somewhat different view-points," and "naturally report somewhat different aspects of it." As Dr. Lloyd put it, "What are we to believe?" Maybe Professor Osborn hopes to pacify a hostile public, for, in the language of Gregory, "in this way sensitive souls may be able to hear the word 'gorilla' without shuddering." However, it should be stated that many who have studied mammalian phyla are in more or less accord with Osborn. Others, like Gregory, disagree.

In a recent address in America, the Lord Bishop of London (Winnington-Ingram) came to the support of science by saying: "Scientists cannot discover too much because no truth can contradict itself." However, to establish truth is not always an easy matter, and to evaluate observed facts properly is equally difficult. One has clearly to do with observations and evaluations. Doubtless, the personal equation enters into the latter to a greater degree than into the former in bringing about varied conclusions.

Mention has been made of Owen. I have always admired this great comparative anatomist. One must not forget the time at which he lived. Then one will be less harsh in criticism of him. Owen believed that the differences between man and ape are so diverse that it is necessary to list mankind in a separate and distinct order in the animal kingdom. In this he erred and had to yield ground to Huxley. Engaged in the controversy one finds Hooker, Russell, Darwin, the Bishop of Oxford and others.

The decisive chapters in Darwin's "Descent of Man" are those which have to do with the evolution, anatomy and function of man's brain. As is well known, Darwin was not a professional anatomist, and he accepted Huxley's views "that there was no structure in the human brain that was not already present in that of the anthropoid." Recently (1927), Prof. G. Elliot Smith, in a notable research, concluded as follows: "No structure found in the brain of an ape is lacking in the human brain, and, on the other hand, the human brain reveals no formation of any sort that is not present in the brain of the gorilla or chimpanzee. . . . The only distinctive feature of the human brain is a quantitative one." Quality is sometimes mentioned as a distinguishing feature; however, probably one never will be able to evaluate the imponderable quality.

The following would appear to be newer and yet fruitful fields for the investigation of man's kinship. Some of the fields have been worked, yet in no instance has the harvest been fully gleaned: (*a*) Histologic investigation of a specific cell or specific areas of the brain in a large number of brains from men and women of various degrees of intelligence and the anthropoids and higher mammals. (*b*) A study of the associational and commissural path-

ways, having in mind size and volume. (*c*) A comparative study of both the brain cortex and the commissural and associational pathways of the amount of supporting tissue (neuroglia) as compared with neurons. (*d*) A study of myelinization in macrocephaly and microcephaly, in the brains of scholars and of the illiterate, and in the brains of the higher mammals. (*e*) The effect of stimulation on the growth of centers and on the myelination of nerve pathways. (*f*) A study of the critical developmental periods of the brain, as has been done in other regions of the body.

To show how anatomic, phylogenetic and clinical studies on the central nervous system may be a factor in determining man's kinship, I wish to refer to the work of Dr. Brouwer. Degenerations resulting from localized experimental lesions in the retina of rabbits, cats and monkeys were studied by Marchi's method. The course and distribution of fibers from the retinal quadrants and the macula were charted. The arrangements were found to differ widely in the three animals, and the "monkey so closely resembled man that the pattern there revealed can probably be employed with little change in the analysis of clinical (human) cases."

It appears to me that the experimental method is an additional way of approaching the problem of man's kinship. Heretofore, the approach has largely been geologic and paleontologic and one of morphologic comparisons. Brouwer's work showed the value of experimentation. Again, take the problem of myelination. Born prematurely at the end of the seventh month, a child will be much advanced, so far as myelination of the fibers of the so-called optic nerve is concerned, over a baby that is held in utero to the end of the ninth lunar month. Here one has the factor of stimulation and in a sense the influence of environment on myelinization. One would like to know whether like environment would result in the same degree of myelinization in a series of experimental animals including the anthropoids and man. Time forbids reference to more phases of the experimental method.

In conclusion, I wish to refer to the critical and meaty book of Herrick, "Brains of Rats and Men." It is well known that there are great gaps in the knowlege of matter, life and mind. They appear unlike each other; yet they are all three in human experience. Herrick found fault with many of the preachments of psychologists, and rightly so. He said, "to the biologist, most of this newest psychology seems like very orthodox physiology." Also, "Consciousness is no more a nonphysical entity than is muscular contraction or any other bodily function, and the fact that it is a different sort of a function which has in the past been submerged in obscurantism should not blind our eyes to the scientific evidence that it is a natural, not a mystical process."

Another suggestive statement from Herrick reads as follows: "Somewhere in the history of primate evolution, during the course of progressive elaboration of the apparatus of cortical associations, sufficient complexity of tissue and plasticity or organization was attained to facilitate rapid learning, the retention of memories of single experiences and the abstraction from these of certain features common to all of them and finally the integration of these common features into symbolic patterns. Symbolic thinking is a new kind of function, though the steps by which it was fashioned can probably be traced, just as we have already succeeded in charting in outline the progressive elaboration of the neurologic mechanisms employed." This suggests the charting of the evolution of symbolic thinking.

Herrick concluded by saying, "The first point which I wish to stress in this connection is that mankind has grown up; we have not merely enlarged and complicated the behavior patterns of rats and monkeys; we have improved upon them and added new patterns not elsewhere known." In other words, Herrick would have it understood that men are bigger and better than rats and monkeys. This conclusion, I believe, is scientifically correct, and to the general public it is much more satisfying.

BOSTON SOCIETY OF PSYCHIATRY AND NEUROLOGY

Regular Meeting, Dec. 15, 1927

HENRY R. VIETS, M.D., *President, in the Chair*

AN ATTEMPT TO ANALYZE THE SUBJECTIVE REALITY SPHERE OF THE SCHIZOPHRENIC PATIENT, AND A CRITICISM OF THE "LOGIC-TIGHT COMPARTMENT" THEORY. DR. HELGE LUNDHOLM.

In testing the validity of a theory concerning a special psychosis, it is imperative to consider all its various reaction types and to determine how the theory can be applied to the explanation of them. The outstanding reaction types of dementia praecox, as analyzed from the psychologic point of view, seem to be the following: The first type (1) is the egotistic, introspective and seclusive patient who is socially inactive because of preoccupation, and whose social attitude involves a minimum of effort. Simulating this type for long periods of time is the so-called, catatonic type (1 a), the one in which the patient, episodically gives up his social passivity, either during emotional excitements or during periods of pseudostupor, both conditions involving social effort. Another type (2 a) is the silly hebephrenic patient who displays a kind of foolish coquettish behavior, motivated, as I think, by an egotistic desire to obtain attention, possibly sexually. The last style (2 b) is the extro-active paranoid patient who displays delusions of persecution and grandeur overtly. In an early phase of the development of this reaction type, there often is an extremely seclusive period in which the delusions are slowly formed and systematized and during which the patient superficially simulates type 1. From this condition there is mostly a gradual development into type 2 b.

In addition to the reaction types mentioned, there is a condition that presents such a distinct picture, that it should be attended to separately: the pseudostuporous condition. The pseudostuporous patient is egotistic, introspective, seclusive and extro-active. His negativistic attitude, as I see it, is a voluntary antisocial reaction and involves positive effort, thus distinguishing it from the effort-free, social attitude of patients in type 1. It is a condition that may be comparable with the sulking of children and adults.

The "logic-tight compartment" theory claims that the schizophrenic person lives in two worlds at the same time; one is the real, the other is entirely imaginary, these two being separate entities and entirely unrelated to each other. This view pictures only a certain type of schizophrenia and, obviously, does not hold true for the paranoid patient, who tries ambitiously to rationalize objective reality in accordance with his delusions. I think that the subjective reality sphere of the schizophrenic person is characterized by a fusion rather than by a cleavage, and suggest that it should be thought of as follows. The subjective reality sphere of the schizophrenic person is a fusion of two kinds of matter; one, entirely a creation of fantasy, the other, objective reality. In this subjective world there are different parts in which objective reality and fantasy fuse in different proportions; there are even parts in which objective reality exists in pure form and others in which fantasy exists pure. This subjective reality sphere of the schizophrenic person may be considered to have a number of concentric layers. The surface layer may be regarded as representing that part of the sphere in which reality exists pure. The following layers, as one passes toward the center, may be regarded as representing a series of parts in which the proportion of fantasy fused with reality gradually increases. In this way one obtains a series of layers in which objective reality is more and more distorted by fantasy, thus becoming more and more unreal objectively. One reaches, at last, a nucleus in the center of the sphere in which the value of objective reality in the fusion is nil, and in which the sphere contains pure fantasy.

This view of the subjective reality sphere of the schizophrenic person can be applied to a wider range of reaction types in dementia praecox. It does not exclude the type whose action-course is described by the "logic-tight compartment" theory. In terms of the "concentric layer" theory, this type would be the one of the person who occupies, in his activities, only the nucleus of the sphere and the outer layer. It is characteristic of the schizophrenic person, that whatever parts of the sphere he may occupy, it is always the lower cognitive-conative dispositions, such as the food-seeking instinct, the tendency to defecate and urinate, that are exercised in the outer layer of the sphere, while the higher, more developed sentiments function in the nucleus of the sphere or in the intermediate layers. This is significant for an understanding of the speed of the dementing process in different types of cases. The types of patients who occupy only the nucleus and the outer layer of the sphere become demented rapidly while those who occupy the intermediate layers become demented fairly slowly. This can be explained by the principle that those who occupy the intermediate layers continue to strive in relation to their social environment and thus continue to suffer resistance from it, resistance which has to be overcome. The effort involved in overcoming resistance is what keeps the mental functions in training and protects them from atrophy. The patients occupying only the nucleus and the outer layer will not exercise their higher sentiments, except in a resistance-free, imaginary world; consequently their higher mental functions will deteriorate rapidly.

To throw more light on the subjective reality sphere of the schizophrenic person, one can review again the different types of cases that have been previously described, and try to determine which layers of the sphere they may be said primarily to occupy. The patient in type 1 occupies, I think, chiefly the outer layer of the sphere and the nucleus. This patient may take care of his daily needs normally, but he seems to be socially disinterested in anything beyond what is neceessary for social intercourse in this connection. He does not involve his social environment in his fantasy. His fantasy is imagination, pure and isolated from his striving in the outer layer. The patient in type 1 a, the catatonic type, probably occupies during the excitements, intermediate layers of the sphere. I think that he then temporarily involves his environment in some delusion and reacts accordingly. Besides that, he also occupies the outer layer and the nucleus of the sphere. The patient in type 2 a, the silly, hebephrenic type, probably occupies, besides the outer layer, both intermediate layers and the nucleus in a vague way. He probably involves his social environment in some or other floating and nonsystematized fantasy, perhaps in an immature "prince charming" dream. The patient in type 2 b, the extro-active paranoid type, is the one, par preference, who occupies the intermediate layers of the sphere. The interpretation of the pseudostuporous condition from the point of view of the "concentric layer" theory offers two alternatives: either the patient involves the social environment he demonstrates against by tenseness and irresponsiveness in some delusion, or he plainly finds such tremendous satisfaction in dwelling in the nucleus of the sphere that any interruption is displeasing to him. In the former case, the patient would be said to occupy the intermediate layers of the sphere; in the latter, he may occupy only the nucleus and the outer layer, and his negativistic attitude would be merely an indication to the persons present not to disturb him. Perhaps both types of pseudostuporous condition exist.

The application of the "concentric layer" theory to the different reaction types of schizophrenia again substantiates the limitation of the "logic-tight compartment" theory. There is only one type of case, type 1, that can be said with fair certainty to fit this theory. Furthermore, one alternative of the pseudostuporous condition, as interpreted by me, may indicate an occupation of the outer layer and the nucleus only, and thus be describable by the "logic-tight compartment" theory. These types, and in addition to all the rest, may be explained by the "concentric layer" theory. The latter theory has then the further advantage that it allows an explanation of the different phases in the progression of the dementia in different types of cases.

DISCUSSION

DR. DONALD GREGG: In a discussion of conscious views Dr. Lundholm's term "fusion" seems to me to be not as apt as the term "blend." Fusion connotes too fixed or too rigid a condition. The term blend implies an easily shifting condition. A sudden improvement has often been noted when a patient has been subjected to a surgical operation or when he has experienced an acute somatic infection. But this improvement disappears when the acute physical crisis is over. How does Dr. Lundholm explain this apparent change in personality in accordance with his concept of personality?

DR. LUNDHOLM: It is difficult to recognize the emotional attitude in pseudostuporous patients because the attitude is most decidedly free of expression, but so much can be said that frequently, when they are out of the stupor and retrospect on the condition, they will make sarcastic remarks showing that they have kept the antagonistic attitude toward society during the stupor. I find Dr. Gregg's suggestion valuable and recognize that the term "fusion" is, perhaps, not the correct one. If Dr. Gregg permits, I will profit by the suggestion and adopt the term "blend." I do not know about the significance of sulking in the history of these cases of schizophrenia. It is an interesting and important problem to find anything, whatever it may be, that would be indicative of the further development in the psychotic history. Some psychiatrists and psychologists speak of curing patients with dementia praecox if the patient is taken in an early stage. What is that early stage? When one knows what is prepsychotically significant of the further development into insanity, then one may be able to treat these people before they have entered the psychosis.

THE CASE OF MARGARET RULE AND ITS RELATION TO THE END OF WITCHCRAFT. DR. E. W. TAYLOR.

The trials and executions, twenty in number, which took place in Salem in 1692, marked the end of the excitement regarding witchcraft in this country, excepting for one further case in 1693. This was the case of Margaret Rule, that occurred in Boston. It is notable from the fact that both the Mathers, Increase and Cotton, took an active part in it and also because Robert Calef, a merchant, attacked the evidence, and through his skilful arraignment of Cotton Mather did much toward bringing the fanaticism to an end. A few cases were reported thereafter, but nothing approaching the excitement of 1692 occurred again.

Margaret Rule, a young girl of obscure parentage, was supposed to have been bewitched by a woman in her neighborhood and forthwith manifested the usual symptoms referable to satanic agency. She became a center of attention for her neighbors, and the excitement was heightened by the interest taken in her condition by the ministers. The setting was complete for hysterical reactions of varied sorts, which forthwith occurred. The usual diagnosis that she was suffering from the effects of witchcraft was made and she was treated by the method then in vogue: prayer and attempts to exorcise the evil spirits possessing her. She finally recovered and, as far as is known, thereafter led a normal life. Viewed from a modern standpoint, the case illustrates a type of psychoneurotic disorder, conditioned by the circumstances of the time in which it occurred, and is analogous to the strange hysterical disorders which occurred following the black death. The case is also of interest as marking the final blow to the religious fanaticism which had made possible the episode of witchcraft in this country. Cotton Mather, who represented, among his contemporaries, the religious fervor of the Puritan in its highest degree, never recovered from the attack made on him by Calef. From a psychopathologic point of view, the whole story of witchcraft demands a much closer study than it has yet received. The case of Margaret Rule is of special value in this connection, since it has been reported in great detail.

CHICAGO NEUROLOGICAL SOCIETY

Regular Meeting, Dec. 15, 1927

JOHN FAVILL, M.D., *President, in the Chair*

MENINGIOMA OF THE OLFACTORY GROOVE: REPORT OF A CASE INVOLVING THE LEFT SIDE. DR. GEORGE W. HALL.

This case history is presented because of a report made by Dr. Foster Kennedy of a number of cases of lesions of the frontal lobe which presented a definite syndrome, consisting of optic atrophy, loss of the sense of smell on the side of the lesion, and papilledema in the opposite optic disk. Cushing recently reported several cases of meningioma in his MacEwen memorial lecture in Glasgow, some of which presented the syndrome described by Kennedy in 1911. He showed one case with unilateral symptoms: especially loss of smell on one side, optic atrophy and blindness, with papilledema on the side opposite the lesion. Cushing elaborated on his successful work in removing these tumors by means of electrosurgery.

This patient was examined first on June 21, 1927. At that time he complained of pain over the left side of the head, disturbed vision in the left eye and loss of the sense of smell on the left side which dated back about six or seven years. The general symptoms consisted of attacks of dizziness, attacks of petit mal, and an occasional loss of consciousness in which he would fall to his knees and then recover himself. He did not have convulsions. There was also a definite history of a remarkable change in personality — from a man who had been manager of a growing concern, active and busy, he became slothful and inattentive. As a complicating factor, he gave a history of having had syphilis about fourteen years before. Examination of the blood and spinal fluid, however, gave negative results. Examination showed loss of vision in the left eye with optic atrophy, and the right optic disk presented nothing abnormal. There were paresis of the right side of the face, a positive Babinski sign on the right side, slight slurring of speech, numbness of the right arm when he was quiet and, at times, certain involuntary movements of the right arm. A roentgenogram of the skull presented nothing abnormal. He was sent to the Mayo Clinic early in August, 1927, where an operation was performed. The first stage in the operation was removal of the bone and exposure of the tumor. A day or two later he developed bronchopneumonia and died.

Reports from different oculists in Chicago showed that both optic disks had been normal until about one year before examination, when changes were first noted in the field of vision of the left eye; this was followed by atrophy; changes in the right eye had not been found at any time. The interesting point in this case is that the tumor was so located unilaterally as not to produce changes in the right optic disk.

ACUTE TOXIC ENCEPHALITIS IN CHILDHOOD. DR. ROY H. GRINKER and DR. THEODORE T. STONE.

This article will be published in full elsewhere.

THE DIFFERENTIATION OF EMOTIONAL RESPONSES IN INFANTS: III. A PROPOSED THEORY OF THE DEVELOPMENT OF EMOTIONAL RESPONSES IN INFANTS. DR. MANDEL SHERMAN.

This paper deals with an interpretation of the author's previous experiments on the emotional responses of infants, and is concerned with the following problems: the specificity of the emotional responses of infants; the relative influence of the behavior of the infant and of the knowledge of the causative stimulus on the judgment of emotions in infants; the influence of the quantita-

tive aspect of the stimulus on the character of the emotional response; the functions of the bodily reactions and of the cry during an emotional response, and the formulation of a theory of the emotion as an adaptive response to specific stimuli.

The experiment showed that observers were unable to differentiate between the emotional responses of new-born infants resulting from four stimuli: restraint of the head and face, dropping suddenly, sticking with a needle and hunger. The character of the response was noticeably different only when the stimulating conditions were varied quantitatively.

The conclusion was reached that the emotional responses of the new-born infant to the types of stimuli employed are undifferentiated, and the success of the individual observer in recognizing and differentiating the emotional character of these responses is due to a knowledge of the causative stimulating conditions. The quantitative aspect of the stimulating conditions may be said to be the most important factor in influencing the character of the ensuing reaction. An apparent change in the character of the bodily movements and of the cry accompanying an emotional reaction may be due to a change only in the intensity of that reaction. For the emotional responses of infants less than 12 days old, the terms "inherent quality" or "inherent character" may well be discarded, since they imply characteristics that can neither be noted nor adequately and precisely described. Conditioning of the emotional responses goes on continually, and the reactions to specific stimulating conditions are often considerably altered with the increase of age and experience.

DISCUSSION

DR. MEYER SOLOMON: Although Dr. Sherman used the term emotion, he really did not discuss the mental phase much. I think that the term emotion should be used in a broad sense to include both emotional feeling and bodily reaction or emotional expression. Watson has dealt not with the mental state, not with emotional feeling, but with emotional expression. Dr. Goodhart mentioned that his pictures showed that there seemed to be some evidence of design in the involuntary movements. I think he was led into a somewhat similar error of failing to differentiate between the emotional expression state and the feeling state. Even though the bodily reverberation may be pronounced there may not be a subjective feeling state. In pathologic laughing, for example, a person may have all the bodily expression of laughing and yet on close examination it will be found that the subjective state is not that of joy. In infants, because they react in a certain way, there is no evidence that they have a subjective state comparable to that of adults. In infants there may be a marked commotional disturbance, a profound physiologic upheaval, the infant's tendency to flight or fight, but there is no evidence that the infant has a definite emotional feeling. Often nothing more than a state of annoyance, discomfort and insecurity is present. Any sudden shock brings about an attempt on the part of the infant to adjustment, which is physiologic, a state of disequilibrium. The embryo in utero must have commotional disturbance if the mother is subjected to vigorous bodily shaking, falls, abdominal injury, etc. It seems to me that much of the work by Watson has dealt with physiologic reactions or commotional disturbances, which express the emotion but does not give any evidence of the subjective feeling state that is present in the infant. In a general way, the work on infants helps to substantiate the fact that the James-Lange theory of the emotions is not by any means true. It shows that one can have a definite dissociation between the subjective feeling state and the physiologic or bodily reaction. From my point of view, unless the subjective feeling state is there, I should not call it emotion. I think that Dr. Sherman's presentation helps to prove that point.

DR. SHERMAN: Dr. Solomon's criticism is valid, but how do we know a person experiences an emotion? Only by asking him. Dr. Beverly and I

studied a great many children. A teacher would bring a child in and say, "This child becomes very angry," but on questioning the child he would say, "No, I don't get mad." When one follows these cases for many months and studies them, the child will eventually say, "Yes, I get mad." One does not know the subjective state of infants, nor does one inquire.

NEW YORK NEUROLOGICAL SOCIETY

Annual Meeting, Jan. 3, 1928

GEORGE H. KIRBY, M.D., *President, in the Chair*

RECKLINGHAUSEN'S DISEASE: A CLINICAL AND PATHOLOGIC REPORT. DR. M. NEUSTAEDTER and DR. JAMES R. LISA.

Case History.—G. C., a colored man, married, aged 21, a porter, was admitted to the Central Neurological Hospital, Aug. 13, 1924, complaining of inability to walk and of incontinence of urine. The illness dated back six years. First he noticed weakness of the left lower extremity then of the left upper, and finally both extremities on the right side became involved. He never vomited, had no headache or rise of temperature. The weakness of the extremities grew gradually worse until he was bedridden or able only to sit in a chair. Shortly before admission incontinence set in. Birth and development were apparently normal. There was no history of convulsions, injuries of the head or surgical diseases. He denied venery, alcoholism and drug addiction. His wife was sterile. The family history, as far as the patient knew, was unimportant; both parents died of unknown causes; he has a brother who is married and well.

Physical Status.—On Nov. 2, 1926, the pupils were irregular and unequal, the right being the larger; both reacted fairly well to light and in accommodation, the left more than the right. Fundi Oculi: the blood vessels were contracted; the disks were normal. The visual fields were undisturbed. Reflexes: those of the upper extremities were markedly exaggerated, particularly on the left side, with wrist clonus and a positive Hoffman sign on the left side, which were slight on the right; the abdominal reflexes were present on the right and abolished on the left side; the cremasteric reflexes were absent; the patellar and achilles jerks could not be elicited because of extreme contractures which could not be overcome; double ankle clonus was present earlier in the disease, according to the records, but absent at this time; there was a double Babinski and Chaddock sign, while the Oppenheim sign was normal, the Gordon sign was absent on the right and questionable on the left; the Mendel-Bechterew and Rossolimo signs were absent. Sensibility: Hyperesthesia was present over the right upper extremity; temperature and pain dysesthesia existed over the lower extremities; vibration sense and deep muscle sense were intact. The patient complained at times of paresthesia in the extremities, but pain was not present. The disturbances of sensibility varied from time to time, the patient being not sure at all times of the temperature sense; once he would mistake hot for cold, and again could not discriminate between temperatures. Toward the end all sensibility was diminished. Motility: There was an incomplete spastic paralysis of the left upper extremity, which was held in flexion with contracture deformity, the hand being claw-shaped; a flaccid paralysis of the right upper extremity was present; spastic paraplegia of the lower extremities with contractures, in adduction, was marked. There was marked wasting in the muscles of the right hand and entire left upper extremity. The cranial nerves were intact. There was incontinence of urine and feces.

Skin: Nodules were present all over the chest, abdomen, posterior aspect of the thorax and lower extremities, varying in size from that of a millet seed to that of a walnut. In addition, there were three large tumor masses, the size

of a hen's egg, one on the outer aspect of the left thigh, one on the left hip and another behind the left ear. They were of soft consistency.

The thoracic and abdominal organs were apparently normal. The blood pressure was systolic 140, diastolic 105; pulse rate, 72, regular, equal on the two sides and of good consistency. Toward the end the pulse rose to 90, with a rise in temperature.

Roentgen-Ray Examination.—There was no evidence of mediastinal involvement; there were: bilateral cervical rib formation, slight general pulmonic thickening, the left dome of the diaphragm was visualized 3 cm. above the right, a reversal of the normal relationship. Definite gross bone lesions were not evident.

Laboratory Studies.—Blood and spinal fluid Wassermann and the Lange tests were normal. The blood picture showed nothing abnormal, except that the polymorphonuclears were only 42 per cent. The urine was normal at all times.

Course.—Toward the end of 1926 the patient began to complain of pain in the chest and legs. The lungs were clear. Beginning in 1927, definite changes in the lung were observed; dulness at the right apex, diminished breathing and rough expiration. The patient became generally weak and emaciated and coughed considerably; the temperature began to rise on April 12, and continued between 100.5 and 102 F. until April 20, when he died.

Diagnosis.—Recklinghausen's disease, disseminated myelitis; pulmonary tuberculosis.

Necropsy.—Neurofibromatosis; chronic pulmonary tuberculosis.

The brain, abdominal and thoracic viscera, except the lungs, did not show any gross abnormalities.

Spinal Cord: All spinal nerves were involved in tumor masses varying from 2 to 1 cm. in diameter. Some were subdural, others extradural, and present in emergent filaments. In the lower cervical region was a large mass, subdurally situated on the left side, causing a marked pressure deformity of the cord. The tumors were soft and of rubber elasticity. On section the cut surface was homogeneous and glazed.

Nerves: All the nerves examined were involved in similar tumor masses. The right vagus was moderately thickened throughout. The left vagus had an extremely large mass extending from the foramen at the base of the skull to the clavicle. The remainder of the nerve was markedly thickened by nodular masses. Both brachial plexuses were similarly and diffusely enlarged by these tumors, as were also the phrenics, abdominal sympathetic chain, and the several cutaneous nerves dissected. Transection of these nerves revealed the same cut surface as in the cord tumors.

Skin: The nodules of the skin were similar grossly.

Histology: The nerve tissue was fixed with formaldehyde; the nerves were stained with hematoxylin and eosin, Bielschowski and Van Gieson stains; cord sections, in addition, were stained with Nissl and Weigert-Pal methods.

The tumor masses of all nerves, the vagus, both brachial plexuses, phrenic nerve, cutaneous nerves, abdominal sympathetic chain, nerve roots and skin tumors showed similar pictures with slight individual variations. The most striking feature was a diffuse overgrowth of fibrous tissue. Some of the fibers were collagenous; others were fine. Small blood vessels were numerous. There was an infiltration of plasma cells in most of the sections, the vagus having the greatest number. In a few areas they were diffusely distributed and collected in cellular aggregations in young connective tissue. The connective tissue appeared edematous in practically all areas. The cord sections revealed no striking changes, with two exceptions; in the lower cervical region on the left side, at the level of the tumor mass which caused gross deformity by compression, the anterior horn cells of that side were markedly decreased in number, the balance disappeared through cytolysis. The only tracts showing degeneration were the columns of Goll, the left pyramidal tract, spinothalamic and spino-

cerebellar tracts and the right outer portion of the spinothalamic and spinocerebellar tracts. The tracts of the upper cervical segments were considerably mutilated while the lower lumbar segments were practically intact.

Summary.—1. The disappearance of the anterior horn cells accounts for the atrophic paralysis.

2. The degeneration of the pyramidal tracts of the cervical region will account for the spasticity of the extremities; yet it is interesting that, in spite of the destruction of the pyramidal tracts above, there was no evidence of secondary degeneration below.

3. In spite of the fact that practically all spinal nerve roots were involved in tumors, pain was not complained of except at the end of the illness.

4. The involvement of the spinothalamic tracts accounts for the dissociation of the temperature, pain and touch sense, but at times the patient was able to discern temperature correctly.

Clinical and Pathologic Report of a Case of Hemorrhage in the Thalamus.

Dr. Walter M. Kraus and Dr. J. H. Scharf.

The finding of a blood clot of unusual location and extent in the brain, in a case of a syndrome which, as far as we know, is undescribed, led to a study by serial sections. Since the description of the classic thalamic syndrome by Dejerine and Roussy, many other thalamic syndromes have been described which, although due to involvement of the thalamus or thalamic region, differ from each other both clinically and anatomically.

Clinical History.—Mrs. F. W., presented a right flaccid hemiplegia, with loss of all forms of sensation and of all deep tendon reflexes on the same side; there was no Babinski sign. One month before the present illness, she had suddenly felt nauseated, had vomited and had had an involuntary bowel movement, but she did not lose consciousness. She noticed that the right side of the body was paralyzed. One month after admission to Montefiore Hospital, New York, she died, that is, two months after the onset of the illness.

Postmortem Observations.—An autopsy was performed a few hours after death. Grossly, the brain showed a defect in the left operculum. There was a hemorrhage in the left thalamus, which was old, moderately organized, roughly piriform, and extended into the caudal end of the outer segment of the globus pallidus, passing through but not destroying the posterior portion of the internal capsule. A section taken 1 cm. higher showed a subependymal softening which just touched the posterior portion of the caudate nucleus. The left half of the brain was smaller than the right.

The brain was cut in a horizontal plane into sections of 50 microns in thickness, and thinner sections were stained for detailed microscopic study. From a gross reconstruction the blood clot was found to be pear-shaped, larger on top at about the level of the roof of the lateral ventricle and tapering down abruptly to a small area in the midbrain below. Craniocaudally, its actual extent was from the upper level of the caudate nucleus to the upper level of the red nucleus, a distance of about 28 mm. The direction of the hemorrhage was downward, backward and mesiad, and the diameter of the blood clot differed at the various levels examined; the actual fronto-occipital extent, however, was from the anterior commissure in front to the middle of the red nucleus behind, a distance of approximately 28 mm.; the side to side extent of the clot was from 10 to 12 mm. at its widest part. The structures involved were the upper part of the body of the caudate nucleus, the middle two fourths of the entire external nucleus of the thalamus, dorsal and ventral parts, including the semilunar nucleus of Flechsig, the middle two fourths of the outer half of the internal nucleus of the thalamus, including the centre médian of Luys, and the subthalamic region. The inner surface of the external segment of the globus pallidus above, the inner surface of the internal surface of the globus pallidus below, and the inner surface of the capsule of the subthalamic body

of Luys in the hypothalamic region were touched by prolongations of the blood clot. Examination of the pons, medulla, spinal cord and cerebellum did not reveal any abnormalities.

The blood supply of the thalamic region, as worked out by Foix and Hillemand, contains five main groups of vessels, which they termed arterial pedicles: (1) thalamotuberian; (2) thalamoperforate; (3) thalamogeniculate; (4) choroidean, and (5) lenticulo-optic. With due regard to the fact that the site of the lesion in the case of a hemorrhage is much more variable than when the lesion is due to cerebral softening, and that it is apt not to correspond exactly to the arterial pedicles of Foix and Hillemand — there would be overlapping into adjacent territories — an attempt has been made to determine which vessel is responsible for the hemorrhage. Although the blood clot occupies in part the territory of each of the pedicles, its greatest extent and disposition is in that of the choroidean pedicle. It is therefore believed that the hemorrhage was brought about by a rupture of one of the anterior choroid arteries.

Comment.—This case belongs in the group of the thalamic syndromes; it has been designated, because of the anatomic involvement by the blood clot, as a hypothalamothalamocaudate syndrome. An attempt has not been made to correlate the symptoms and signs with the pathologic observations for the physiology of the region affected is as yet not definitely known. It is noteworthy that in this case, with hypothalamic involvement, there were no visceral symptoms or signs.

DISCUSSION

Dr. Henry Alsop Riley: A great deal of the importance of this communication depends on the chart, and I think it will be only by careful examination and study of this detailed comparison of the symptoms and signs in the various kinds of thalamic lesions that one can gain the most from this report. Dr. Scharf and Dr. Kraus should be congratulated for the preparation of this study which has been well made. The clinical history and report of the physical examination as reported tonight were sketchy and many items of importance were left unmentioned. Astereognosis has often been discussed before this Society, and it has always seemed to me that the use of the term should be limited to a sensory disturbance in which the object cannot be recognized by the sense of touch, when the primary sense qualities are normal or relatively normal. In this case the primary sense qualities of touch, pain, temperature and others were much disturbed, so that there was a fundamental lack of the proper constituents for cortical sensation and therefore not a true astereognosis; it was a real hypesthesia. I find it difficult to explain why there was such an extensive hemiplegia with practically no pathologic change in the pyramidal tract and in the part of the capsule through which the pyramidal tract passes. I should much like to see sections of the brain stem and the spinal cord. The pyramidal tract must have been injured or compressed because the patient had a hemiplegia; it is hard to attribute a complete loss of motor function to a sensory disturbance or to a basal ganglion lesion.

Another point is the denomination of the syndrome on the basis of the pathologic observations. Dr. Scharf has called this a hypothalamothalamocaudate syndrome. So far as I can determine, except for the initial vegetative disturbance with the onset of the hemiplegia, this patient did not show any material hypothalamic symptomatology; furthermore, no mention was made of any symptomatology which could be traced to the caudate, so that although it is evident from the pathologic changes that the name of this syndrome is correct, from the symptomatology a thalamocapsular type of diencephalic hemorrhage would probably have been better.

Dr. Armando Ferraro: The case is certainly interesting and has been carefully studied, but as Dr. Scharf has pointed out, a case of hemorrhage is not the best means of increasing knowledge of the thalamic syndrome. The cases in which softening occurred are better suited for this purpose as the

accompanying manifestation of diaschisis is less pronounced. The blood supply of the thalamus is complicated, and French authors, especially Foix, Hillemand and Masson have contributed greatly to this problem. The author localized the hemorrhage in the territory of the choroid pedicle. I do not really think that we can locate exactly the hemorrhage in any of the five most important pedicles, as the lesion involves also the territory of the thalamoperforate and of the thalamogeniculate pedicles.

An important point in the account is the absence of deep reflexes and of a Babinski sign in the presence of a lesion of the internal capsule. I do not know if the examination of the spinal cord has been made and if the spinal roots have been studied histologically. Cases have been reported in which, following lesions of the central neuron, there were lack of increased deep reflexes and muscular atrophy which facts have been correlated with involvement of the spinal roots. As in this case there was a general diffuse arteriosclerosis, the disappearance of reflexes might be correlated with changes in the radicular system. Other authors who have studied the occurrence of flaccid paralysis and muscular atrophy in lesions involving the central neuron claim that these facts occur when the basal ganglia, especially the lenticular nucleus, are involved (Mingazini). In this case, the involvement of the lenticular nucleus may be taken into consideration for such an explanation.

I agree with Dr. Riley in opposition to the denomination of the syndrome as hypothalamic. In the clinical history there was no mention of any particular sign pointing to involvement of the hypothalamus. The cerebellar signs and the peculiar type of intentional tremor which have been described by Chiray, Foix and Nicolesco in their "superior syndrome of the red nucleus," namely, a lesion located between the thalamus and the red nucleus are lacking in this case and make it difficult for one to consider the syndrome as hypothalamic.

Dr. Michael Osnato: Was the diagnosis of uremia corroborated by repeated studies of the blood chemistry and by careful consideration of the medical side of the situation? The reason why I ask is that there is the possibility, aside from the considerations which Dr. Ferraro has just mentioned, that some one might say that the diminished reflexes and the flaccidity were part of a focal edema of the brain, such as one often encounters as a terminal condition in uremic states. A massive, one-sided weakness in uremia due to large edema in the brain is common and in this condition the flaccidity is often striking; it is also common to find diminished or even absent reflexes. When the report of this case is published, the question of whether this was uremia will be of considerable importance. From the blood chemistry and urinary observations recorded or at least referred to here, one is not justified in making a diagnosis of uremia. The right-sided convulsions (opposite to the choreo-athetoid movements) may have been simply the convulsions of an acidosis of nephritic origin and not necessarily a uremic manifestation.

Dr. Walter Kraus: Dr. Riley has commented on the involvement of the internal capsule. Beyond what we were able to show in the sections (a slight paling of the capsule in the region of the hemorrhage), it was not involved. There was no involvement in the spinal cord. Dr. Riley has emphasized that one would have expected it. We did, but we did not find it. I saw this patient before admission to the hospital, and I also cut the brain post mortem. I was much surprised to find that the capsule was not involved grossly, and not beyond what has been demonstrated microscopically. Therefore I differ with Dr. Riley in his suggestion that the name which has been applied to this syndrome be altered.

Dr. Riley believes apparently that hypothalamic syndromes of arterial origin must have visceral disorders. That is not true. Hypothalamic involvement due to hemorrhage has been described with reasonable frequency without disorders of visceral function.

Dr. Osnato's question was interesting because this patient had a convulsion on the side opposite to the paralysis, without any corresponding pathologic

lesion. It was toxic in origin and was due to uremia. Pathologically, the side of the brain opposite to the hemorrhage was examined carefully because we wanted to account for this other manifestation, but nothing was seen.

These cases may be considered from: (1) a pathologic point of view; (2) a point of view of the blood supply, and (3) a purely clinical point of view. As far as I know in practically no instance can these three be reconciled at the present time. A syndrome must be named anatomically or clinically, or by its vascular involvement. In my mind it is impossible to reconcile the three kinds of descriptions which one might make of such a lesion of the thalamus, the reason being that one is not sure of the exclusive limitations of the blood supply. One knows to some degree at least what it includes, but not what it excludes. One is not entirely sure of many details of the functions of the thalamus. Therefore, one is left without accurate knowledge of the blood supply and without accurate knowledge of the physiology to correlate with what one may observe in patients. I think, therefore, that there is ample justification, until one knows more about the blood supply and the functions of the thalamus, to give a syndrome a name on a purely pathologic basis, particularly when one has the opportunity to examine a brain in serial section with the completeness with which Dr. Scharf has prepared the one presented by us tonight.

THE INFLUENCE OF MALARIAL TREATMENT ON THE PATHOLOGIC CHANGES IN GENERAL PARALYSIS. DR. ARMANDO FERRARO.

This article will appear in full in a later issue.

CHLORIDE BROMIDE TREATMENT OF PATIENTS WITH EPILEPSY (WITH LANTERN SLIDE DEMONSTRATIONS). DR. J. NOTKIN.

This article will appear in full in a later issue.

Book Reviews

DIE RHINOGENEN AKTIONSSTRÖME IM VEGETATIVEN NERVENSYSTEM UND IHRE REGULIERUNG. By SAN.-RAT DR. A. FRÖSE, Facharzt für Nasen-, Ohren- und Halsleiden in Hanover. Price, 2.80 marks. Pp. 71, with 3 illustrations in the text. Berlin: S. Karger, 1927.

In this monograph, the author, a specialist in diseases of the ear, nose and throat, makes a bold attempt to invade the field of general medicine. In the introduction he states that he desires "to call to the attention of science that there exists an undoubted causal relationship between pathologic conditions of the nose and the vegetative nervous system." He also states that his thesis is based largely on hypotheses which, when accepted, will show that "the development of various clinical pictures in the course of curable nasal reflex diseases depends on the effect of the rhinogenous action current and its varying intensity and localization." According to the author, the nose affects the course of all biologic processes, psychic as well as somatic, emanating from the autonomic nervous system. It is a neurohormonal regulator in most intimate connection with the vegetative nervous system. Nasal neuritis (whatever that may be) is an integrating component of the reflex neuroses; as a result of release of tension associated with nasal neuritis the latter exercises a whiplike stimulating effect on the vegetative centers, giving rise to dysfunction of this system which can be overcome by removing the neuritic (?) irritation, which in turn causes the irregularities and excesses of the pathologic action current to disappear. When all this has been accomplished, all functional disturbances and those of their sequelae that are still reparable, rapidly vanish. Both for diagnosis and as an adjuvant to the local treatment of the nasal neuritis, the palpatory control of the existing rigidity of the abdominal muscles is a sine qua non.

This, in short, is the author's thesis. Twenty-six pages are utilized to elaborate it, after which fifteen cases are reported in detail to illustrate its validity. One of the patients had been suffering for years from epigastric distress, nausea, constipation, loss of appetite and a sensation of tightness in the neck, chest, and around the heart. There were no complaints referable to the nose. Examination showed increased cardiac dulness (the patient had been suffering from heart disease for thirty years—this is in parentheses), a systolic blow over the heart, hypertonicity of both abdominal recti with myalgia during palpation; the nasal mucous membrane was hyperemic, and there was a spur on the right side of the nose with extremely hyperesthetic neuritis (?) lesions on the left side. Treatment of the lesions in the nose was followed, in five days, by relaxation of the abdominal hypertonicity, all pains and complaints disappeared. It is not stated whether or not the cardiac murmur also vanished with such rapidity.

Most of the author's clinical material is of a similar nature, and the results of his therapy are just as startling. In reading this monograph one is reminded of the wholesale colectomies performed about two decades ago in our own country for the cure of epilepsy, and the more recently reported brilliant cures for dementia praecox and the manic-depressive psychoses by the extraction of teeth, tonsils, adenoids, resection of the colon, appendectomy, oophorectomy and hysterectomy, not forgetting the restoration of idiots to mental normality following the administration of endocrine substances and hormones by enthusiastic endocrinologists, as well as the cure of epilepsy, multiple sclerosis and arterial hypertension by psychoanalysis.

How can an author, who apparently has a better grasp of general medicine and hormones than the average nose and throat specialist, so dissociate himself from logic and ordinary clear thinking? It must be overenthusiasm that gives

him such a constricted mental visual field. This criticism may appear unduly harsh, but in the reviewer's opinion this booklet is no contribution to medical literature. Even the most ravenous seeker for truths in medicine will lose nothing by not reading it.

NERVE TRACTS OF THE BRAIN AND CORD. ANATOMY: PHYSIOLOGY: APPLIED NEUROLOGY. By WILLLIAM KEILLER, F.R.C.S., Ed., Professor of Anatomy and Applied Anatomy at the University of Texas. Price, $8.00. Pp. 436. New York: The Macmillan Company, 1927.

As the author states in his preface "this book on the nerve tracts of the brain and cord is the result of 20 years' experience in teaching the anatomy of the brain and cord in such a manner as to enable students to approach nervous diseases, thinking in terms of anatomy, physiology and pathology, as applied to the nervous system.

"Part I supplies a laboratory manual for the study of the nerves and tracts in the central nervous system as they may be demonstrated in the cord and brain stem in the adult and foetus and in pathological specimens stained for myelin, as well as in early degenerations stained by the Marchi technique.

"Parts II and III form the basis of a course of thirty lectures giving, in Part II, a summary of the anatomy and physiology of the nerve tracts, mainly based on newer methods of investigating the autopsy findings in clinical cases, and in Part III, the leading features of the better known nervous diseases, correlating their symptomatology with anatomical, physiological and pathological data.

"No effort is made to go into details and only such leading facts are emphasized as should make an appeal to every well-educated physician." The foregoing statements, quoted verbatim, express the purpose of the book.

What is the result? The first part, which consists of about 100 pages, is written well, the description of the anatomy being adequate, although short. There are no illustrations in part I.

In the second part, chief emphasis is laid on physiology. It consists of about seventy-four pages with two illustrations. At the end of the discussion of part III, which consists of about 120 pages, there is a supplement in which an attempt is made to classify the symptoms of various organic diseases. Following this there are several tables, and then the illustrations, which begin on page 333 and end on page 432, including a colored drawing of the sympathetic nervous system, and finally a large and excellent drawing showing all the cortical spinal tracts in one large illustration.

There are several interesting features in this book; one is the placing of the illustrations and diagrams separately from the text. With few exceptions, most of these are borrowed. The author deserves credit, however, for the large illustration already described, which is extremely well done. Another unusual feature is the tabloid classification of diseases from an anatomic standpoint. This furnishes a rather rapid means of review. The subject matter is presented shortly and concisely, perhaps too much so. It is a question whether the book by itself will fulfil its purpose. With the actual lectures by the author it undoubtedly does.

LES SYNDROMES NEVROPATHIQUES. By DR. A. HESNARD. Price, 40 francs. Pp. 247. Paris: Gaston-Doin et Cie, 1927.

This volume is one of several devoted to special syndromes published under the direction of Roger. The author, well known for his work on psychoanalysis, has assembled a group of minor mental maladies consisting of neurasthenia, psychasthenia, obsessions, neuroses and hysteria. A description of the clinical manifestations of each type with differential features and diagnosis takes up a large part of the book and is done in a detailed and clear cut manner and is almost characteristic of the French. The wealth of detail at times is almost

baffling. Two sections on the pathologic significance and treatment for these neuropathic syndromes are well worth reading. Hesnard considers all neuropathic syndromes to be of common origin. They develop or remain latent during infancy, they break out at various periods of stress and tend strongly to relapse. In their rise and fall they run almost parallel with the rise and fall of organic and psychic sexual existence. They indicate, in general, a lack of satisfaction in the affective sphere. Much can be blamed on heredity, some on infectious disease, injuries at birth, and much on the surroundings during the first period of life. Striking occurrences or accidents in the course of life are apt to be the exciting rather than the predisposing factors.

The psychopathology of the neurasthenic syndrome is interpreted as the first degree of irruption into the affective consciousness of the subject of a morbid irritation of the emotional and cenesthesic apparatus.

Psychasthenia, according to the author, has an abnormal character revealed by the constitutional development of certain conscious morbid sentiments, late results of a disordered evolution of the instinctive life which prevents the person not only from perceiving and adjusting to reality but also from harmonizing himself emotionally to a sufficient extent for practical and social existence.

The anxiety neurosis seems to be the clinical manifestation of a peculiar hyperexcitability of the emotional apparatus in the psychic defense against external and internal danger. This hyperexcitability is brought about by sudden dissatisfaction of certain instinctive tendencies like the infantile egotistic need for possession and protection. In the adult, the anxiety neurosis arises particularly from unfulfilment, notably of sexual necessities, "particularly when they are effectively solicited without being completely satisfied."

Obsessions are psychic parasites, conscious but enigmatic in their far away origin in the life history of the individual, and they act as barriers between everyday life and the memories, imaginations and aspirations of infancy. The obsessing idea is a "hernia of the unconscious into the conscious."

Hysteria is a neurosis characterized by the inability of the person to assimilate impressions which remind him of certain disagreeable events unbearable to his exaggerated self-love.

BEITRÄGE ZUM TICPROBLEM. By DR. J. WILDER and DR. J. SILBERMANN. M. 8.40. Pp. 100. Berlin: S. Karger, 1927.

In the introduction to this monograph on tics the authors state that they have not been more successful than their predecessors in an attempt to clarify "this perhaps forever dark chapter in medicine" as to the relationship between "soma and psyche," or what should be better designated as "motility and psyche." The monograph is divided into three parts. Part I is devoted to a discussion of the problem of tics in the light of modern views on this subject with special attention to the striatal hyperkineses. Part II contains the reports of fifty personally observed severe clinical cases of organic and functional tics, a critical review of some cases culled from the literature, and a poorly indexed bibliography. Part III is a detailed report of a case of maladie des tics. Part I contains thirteen chapters, part II eight chapters and part III one chapter.

The second chapter in part I is devoted to definition, classification and differential diagnosis, and to a critical review of previous attempts to define tics. The authors' definition consists of twenty-three lines in five typically German long sentences. "This pseudodefinition" (as they call it) they admit "is not satisfactory from a theoretical point of view but will serve as a good working definition for practical purposes."

The third chapter deals with statistics with a special description of tics in children, varieties of tics, and psychic habitus. This is followed by a chapter on war tics. The next chapter is devoted to organic tics, striate hyperkinesias, which the authors rightly emphasize are in most cases impossible to distinguish from functional tics.

Chapter VI contains a discussion of functional tics. "Although," they say, "there exists no exact proof that mental tic has any relation to the basal ganglia,

nevertheless there is abundant justification for such a hypothesis." After this the authors plunge vigorously into a discussion of psychoanalysis and tics. This discussion is anything but complimentary to the freudians, both "orthodox and reformed" (reviewer's quotation marks), but it has the merit of being unbiased and sane. They even go so far as to admit that they too believe that tic is a conversion symptom, but in their opinion it must be separated from hysteria even though the border lines between the two conditions are extremely hazy. This is by far the best discussion in the entire monograph, but unfortunately they spoil its effects in the next chapter in which they describe what they call their own psychoanalytic methods, which as far as the reviewer can see, are neither psychoanalytic nor anything else. Their methods can serve only one purpose; they will offer an opportunity to the psychoanalysts to proclaim loudly and justly that the authors do not understand psychoanalysis.

These comments indicate the scope of this little brochure, and although it contains nothing that is new or original, it is a good resumé of the subject and as such is recommended to all who are interested in it.

UNDERSÖGELSER OVER NOGLE AF BLODETS ELEKLROLYTER (Ca, K, Na, H) OG DET VEGETATIVE NERVESYSTEM, SÄRLIG HOS PATIENTER MED MANIO-DEPRESSIV PSYCHOSIS. By HELGI TÓMASSON. Paper. Pp. 257. Copenhagen: 1927.

This large monograph in Danish by an Icelandic author is a doctorate thesis defended before the faculty of the University of Copenhagen. Chemical studies in connection with neuroses and psychoses have been much in vogue in Denmark. The most extensive recent investigation was that on "dysregulatio ammoniaci" in epilepsy by Bisgaard, Larsen, Nörvig and others. Tómasson, supporting himself on the work of Jacques Loeb, Howell and others on the effect on cell activity of the electrolytic concentrations and relations of the surrounding fluids, raises the question whether patients with certain mental disorders associated with distinct vegetative symptoms may present alterations of the kat-ions of the blood, particularly the calcium and potassium ions. His studies in cases of manic-depressive psychosis led him to answer this in the affirmative. The next problem was to determine whether these changes in the electrolytes of the blood can be explained as results of alterations in the vegetative nervous system, or whether the vegetative alterations depend on the changes in the electrolytes. The investigations carried out concerned chiefly: (1) the electrolytes of the blood, especially calcium, but also potassium, sodium and hydrogen, in normal and psychotic persons; (2) the condition of the vegetative nervous system in manic-depressive patients, and (3) the calcium, potassium, sodium and hydrogen ion concentration in experimental vagus palsy. One of his objects was to learn whether the fluctuations in the electrolytes could be explained by the theory of Howell, further elaborated by Zondek, according to which the sympathetic nervous system mobilizes the calcium as needed, and the parasympathetic system the potassium and sodium. The chief conclusions are: (1) The humoral instability existing in manic-depressive and other functional psychoses is characterized by an abnormal fluctuation in the electrolytes of the blood. This causes fluctuations in the general neuromuscular irritability, especially in the vegetative nervous system, and the fluctuations are below the normal level as increased neuromuscular irritability does not occur in these conditions; (2) the fluctuations in the electrolytes in the blood in manic-depressive patients cannot be explained by the Howell-Zondek theory. On the other hand, the local variations in the irritability of the vegetative nervous system appear to be connected with the alterations in the electrolytes in accordance with Loeb's demonstration that the calcium-sodium quotient determines the general neuromuscular irritability. The author points out that the work on tetany and the results of calcium treatment in this disease also confirm the correctness of Loeb's theory, that while an increase in calcium lowers the irritability an increase in sodium raises it. While in tetany the calcium content is low it is relatively high in the functional

psychoses, and the hydrogen ion concentration has a tendency to displacement to the alkaline side. It is noteworthy that the finding of similar humoral conditions in the manic and depressive phases offers new support to the correctness of the conception of the manic-depressive psychoses as an entity.

NOUVEAU TRAITÉ DE MEDÉCINE. XXI. NERFS-SYMPATHETIQUE-NEVROSES. Published under the direction of DRS. G. H. ROGER, F. WIDAL and P. J. TEISSIER. Price, 85 francs. Pp. 887. Paris: Masson et Cie, 1927.

This book is the twenty-first volume of the new treatment of medicine which has been appearing within recent years and is edited by Profs. G. H. Roger, F. Widal and P. J. Teissier. Some of the previous volumes have been reviewed. The present volume deals with the peripheral nerves, the sympathetic system and the neuroses.

Traumatic conditions of the peripheral nerves, the symptomatology of the peripheral nerves and plexuses, the radicular syndromes and radiculitis, polyneuritis and neuralgias; this part which comprises 405 pages is by Tinel. American readers are already familiar with Tinel's work which has grown out of the World War. Many of the illustrations used are in his previous work. Altogether, in these pages is an excellent presentation of the modern point of view of diseases of the peripheral nerves.

The sympathetic system is treated in six divisions. These subdivisions are interesting. There is first the syndromes of the neurovegetative system, then their vasomotor difficulties; then trophic disturbances which are discussed not only by Heuyer, but in a special chapter by Marinesco, whose discussion is chiefly devoted to the arthropathies occurring in cerebral and spinal diseases. Then follows a chapter on thermic disturbances whose origin is in the nervous system. So far as the reviewer knows this subject has never been treated so distinctively; and finally there is a discussion of migraine.

In the group which is headed neuroses and dyskinesias, there is first an introduction to the neuroses. Then there follows in order a discussion of epilepsy, hysteria, neurasthenia, traumatic neuroses, choreas (which are subdivided into acute and chronic, myoclonias, spasms, tics, functional cramps, tetany, and lastly, the familial diseases of the nervous system.

The authors are well chosen and in most instances the discussions are adequate. This volume is a creditable presentation of the French point of view.

THE CAUSE AND CURE OF SPEECH DISORDERS. A TEXT BOOK FOR STUDENTS AND TEACHERS ON STUTTERING, STAMMERING AND VOICE CONDITIONS. By JAMES SONNETT GREENE, M.D., and EMILIE J. WELLS, B.A. Price, $4.50. Pp. 441. New York: The Macmillan Company, 1927.

This book is intended for the physician and for the patient who is subject to speech disorders. It is divided into four parts: (1) general introduction with a discussion of the person who stutters, the person who stammers and the person whose voice is abnormal; (2) a discussion on stuttering; (3) stammering, and (4) the voice. According to the authors the first essential in the study of disturbances of speech is to determine whether one stutters, stammers or has a voice condition. This classification is made necessary both from the therapeutic and curative point of view. "Stuttering is a speech of a hesitating nature which is conditioned on certain states of mind in the form of emotions, feelings, attitudes or ideas." Stammering is defective speech resulting either from central or peripheral involvement. As Gutzmann has expressed it, stuttering is a defect of conversation, while stammering is a defect of enunciation. The authors insist that stuttering is the result of a psychic abnormality and that it cannot be cured by the different mechanical methods employed, and that the only way to obtain results is to remove the original psychic cause of the first attack and to reeducate the person.

The authors make some interesting statements; namely, that nine out of every ten stutterers are slow to move and slow to think; that stutterers as a

class inherit a nervous constitution, and that an emotional feeling in them has more than an average reaction. There is an excellent description of the psychology of the stutterer, of the development of the sense of inferiority and so on. The treatment consists in training for better coordination, better balance of the functions that compose the making of speech and a reeducation of the person.

There is an interesting chapter on the cost of defective speech. Both the individual and the community loses. They estimate that there are about 400,000 stutterers in the United States and that the total loss to the community is over six billion dollars during their lifetime. The part on stammering is interesting. The methods used in curing such disorders is well elaborated. Lastly there is a discussion of the voice and how its abnormalities can be improved. Altogether it is an extremely instructive book and one qualified to help the person who suffers from such speech disorders.

QUEEN SQUARE ITS NEIGHBOURHOOD AND ITS INSTITUTIONS. By GODFREY HEATHCOTE HAMILTON, Secretary, National Hospital, Queen Square. Price, post paid, $2.75. Pp. 146. London: Leonard Parsons, Devonshire Street, 1926.

All American neurologists, especially those who have had some training in the National Hospital, Queen Square, will be grateful to Godfrey Heathcote Hamilton for this delightful book on Queen Square, its neighborhood and its institutions. Mr. Hamilton is eminently qualified to do this work inasmuch as he has been secretary of the hospital for over twenty-five years. While most of the book gives the history of Queen Square, neurologists will be most interested in the chapter which deals with the National Hospital, which opened its doors in June, 1860, at which time the first in-patient was admitted to the hospital. Its work is too well known for any extended comment.

Following the history of the hospital is a list of its staff, among whom are the most famous names in neurology. The illustrations are delightful. Every neurologist should have a copy which can be obtained by writing directly to the National Hospital, Queen Square, London, England. The profits of the sale of this book go to the funds of the hospital.

DREAMS. By PERCY G. STILES, Harvard Medical School. Price, $1.50. Pp. 80. Cambridge, Mass.: Harvard University Press, 1927.

It is an unusual thing for a person to record and illustrate his dreams, yet this is what Dr. Stiles has done. The author has been collecting his dreams for a period of thirty years and is still recording them. He evidently derives pleasure from it. During the last ten years he has been making sketches of them. Some of these illustrations were made many years after the date of the dreams.

The discussion is divided into the sensory content, bodily states, memories and anticipations, the emotional content and the personality of the dreams. The author does not claim to be a psychologist. Nevertheless, he has definite ideas as to the psychology of his own dreams. It is always a hazardous thing for a person to interpret his own dreams and it is questionable whether one can do so—it is, however, an interesting record. Particularly entertaining are the sketches.

NERVENSYSTEM UND SPONTANE BLÜTUNGEN MIT BESONDERER BERÜCKSICHTIGUNG DER HYSTERISCHEN ECCHYMOSEN UND DER SYSTEMATIK DER HÄMORRHAGISCHEN DIATHESEN. By DR. RUDOLF SCHINDLER, Facharzt für innere Krankheiten in München, früheren Assistenten der Abteilung. 5 illustrations in the text. Price, 4.20 marks. Pp. 68. Berlin: S. Karger, 1927.

This monograph consists of four parts. The first part deals with the relationship of the nervous system to spontaneous bleedings; the second with

spontaneous ecchymoses in organic diseases of the nervous system, the third with spontaneous ecchymoses in the psychoneuroses, and the fourth with the nervous system and the hemorrhagic diatheses. The contribution contains nothing original, but is an excellent review of the present knowledge of the subject and with its 113 references to the literature, it may be read with profit by any one interested.

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VOLUME 19 NUMBER 6

ARCHIVES OF NEUROLOGY AND PSYCHIATRY

JUNE, 1928

PUBLISHED MONTHLY BY AMERICAN MEDICAL ASSOCIATION, 535 NORTH DEARBORN STREET, CHICAGO, ILLINOIS. ANNUAL SUBSCRIPTION, $8.00

Entered as Second-Class Matter, Jan. 7, 1919, at the Postoffice at Chicago, Illinois, Under the Act of March 3, 1879. Acceptance for mailing at special rate of postage provided for in Section 1103, Act of Oct. 3, 1917, authorized Jan. 15, 1919.

THE ARCHIVES OF NEUROLOGY AND PSYCHIATRY is published by the American Medical Association to stimulate research in the field of diseases and disorders of the nervous system, and to disseminate knowledge in this department of medicine.

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